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Science and Education in the Washington Area

BY FRANK L. CAMPBELL

President, Washington Academy of Sciences

(A radio address delivered on *Engineering News-Report*, March 1, 1959)

Yesterday in Washington three scientific lectures for high school students and teachers were given at 10:30 a.m.: one by Dr. Raymond J. Seeger, of the National Science Foundation, entitled "Faster Than Sound"; the second by Dr. R. Kenneth Lobb, of the Naval Ordnance Laboratory, on "Hypersonic Flight in the Frontiers of Space"; and the third by Dr. Sterling B. Hendricks, of the U.S. Department of Agriculture, on the "Action of Light on Plants." They spoke at the Carnegie Institution of Washington, the U.S. National Museum, and the National Institutes of Health, respectively. All three are outstanding scientists, particularly Dr. Hendricks, who is a member of the National Academy of Sciences and who recently received an award from the President of the United States in recognition of his scientific work.

Why were these distinguished men lecturing on a Saturday morning when they might have been resting? Because, like hundreds of other scientists and engineers in the Washington area, they feel that it is their civic duty to give freely of their time and experience in order to transmit to younger people some understanding of their work and of their enthusiasm for it. Call it free advertising, if you like, for these men are selling science and engineering to students who have yet to choose a career. They do it not only because they know that our country needs an ever-increasing number of persons skilled in science and technology but also because they believe in their work as a deeply satisfying way of life. So they speak partly as missionaries, trying to convert those who do not understand the ethical values of work that requires great devotion and absolute honesty.

The lectures just mentioned were part of a series of nine. How were they arranged and how in general is the great scientific and engineering talent of the Washington area brought to bear on young people? It is done by joint effort of two federations of local societies; one is called the Washington Academy of Sciences, the other the D. C. Council of Engineering and Architectural Societies. The Academy unites 26 scientific and engineering societies; the Council 29 engineering and architectural societies. Six engineering societies are common to both. It would have been chaotic if each of the 49 local societies had tried to do its own proselyting in the schools of the Washington area; even independent action by the Academy and the Council would have been inefficient if not irritating. The problem was solved in 1955 by the creation of the Joint Board on Science Education; i.e., joint between the Academy and the Council, but to a considerable degree independent of both. The Joint Board has many other functions beside the arrangement of the lecture series first mentioned. Its most remarkable feature is its Secondary School Contacts Committee. This is an organization of eight divisions covering junior and senior high schools in six areas in and around the District of Columbia as well as parochial and private preparatory schools in the whole Washington area. For each of the 150 schools of the area there is a scientist or engineer who had volunteered to help the science and mathematics teachers and students of his chosen school in any way he can. Through the Contacts Committee he may get Career Day speakers, speakers for science clubs, films on science and engineering topics, advice and assistance on science projects, and assistance for local science fairs. Occasionally the science or mathematics teachers of the area wish to go to national science teachers' meetings on school days. Then the Contacts Committee arranges for their classes to be taken by professional scientists and engineers, who, though inexperienced in teaching, are willing to brave hostile or indifferent students in order to carry their message to the students willing to listen.

I could tell you of many other achievements of the Joint Board and of its parents, the Academy and Council, but time is lacking. We scientists and engineers in the Washington area are proud of what we are doing for our successors. Looking at the fine membership of our Junior Academy of Sciences, at the science fairs held here, and at other evidence of scientific interest and competence of many of our young people, we are optimistic about the future of science and engineering in this community.

PHYSICS.—*Men and electrons.*[1] L. MARTON, National Bureau of Standards.

(Received February 18, 1959)

Several months ago I came to the conclusion that the history of the electron (or of electrons) might be a good subject for the address of the retiring president of this society. In the meantime, I had ample opportunity to study the subject and found, to my surprise, that there is even more historical material than I ever imagined. Under the circumstances it seems preposterous to add one more history of the electron to the existing ones unless one can say something new. I take courage, however, from the fact that my interpretation of one or the other phase of the development does not seem to agree entirely with those derived from earlier papers. I present here, therefore, a slightly different version of the history of the electron or, to be more precise, some fragments of the history of the electron.

To start, I should like to show a tabulation of the properties of electrons, as we know them now. Though incomplete this represents a few of the salient facts we have acquired in the past and allows us to look at the different aspects of the behavior of the electron. At the same time this permits us to group our discussion around certain conceptual aspects of this development, and the first of these conceptual aspects is the

PROPERTIES OF ELECTRONS AND RELATED DEFINITIONS

CHARGE	$e = \pm 4.8 \times 10^{-10}$ e.s.u.
REST MASS	$m_0 = 9.1 \times 10^{-28}$ g
MASS	$m = m_0[1 - (v^2/c^2)]^{-1/2}$
RADIUS (CLASSICAL)	$r = e^2/mc^2 = 2.8 \times 10^{-13}$ cm
VELOCITY	v
MOMENTUM	$p = mv$
KINETIC ENERGY	$E = c^2(m - m_0)$
WAVE LENGTH	$\lambda = h/p$
SPIN	$s = \frac{1}{2}$
MAGNETIC MOMENT	$\mu = 0.93 \times 10^{-20}$ erg/gauss
STATISTICS	Fermi

corpuscular nature of the electron. We shall try to examine, therefore, how the concept of the elementary charge emerged and who are its originators. In other words, my presentation of the history of the electron will not be a strictly chronological one. However, within the framework of whatever conceptual device I may follow, I shall use a more or less chronological development.

It is customary to start the history of the electron with the famous utterings of Benjamin Franklin. These appeared first in 1756 and are as follows: "The electrical matter consists of particles extremely subtle, since it can permeate common matter, even the densest metals, with such freedom and ease as not to receive any appreciable resistance." This statement is usually taken as a clear proof that Benjamin Franklin for the first time conceived the idea of electricity consisting of very small particles and was aware of this corpuscular nature of electricity. I can not agree entirely with that viewpoint. Let us examine this statement in the light of what was known when Franklin announced it and what kind of background existed. In those times the commonly accepted theory described electricity in terms of two fluids; one so-called vitreous and one resinous. In case someone should be inclined to take this 2-fluid theory as a precursor of the present conception of electrons and of positrons, I must warn you that the two fluids had to be present in equal quantities to describe the phenomena known at that time. Franklin took the very important step of replacing this 2-fluid theory with a 1-fluid theory in which all known phenomena could be described in terms of one type of electricity or by the lack of it. This was a tremendous step forward, and its importance can not be sufficiently underlined. It paved a way toward our modern conception of electricity, and indeed it paves a way toward the electron concept, but—and here comes a very important "but"—the fact that the description was in terms of "particles" does not have the same meaning as when we talk today of particles of electricity.

In a chapter of the first edition of the Handbuch der Physik, Walter Gerlach compared the role played by Franklin in the development of electron theory to that of Democritos in the development of the atomic theory of matter. I would agree par-

[1] Address of the Retiring President of the Philosophical Society of Washington, delivered on January 16, 1959.

tially with this viewpoint. Both Democritos and Franklin used the atomistic concept in describing certain phenomena: Democritos, those of matter; Franklin, those of electricity. Neither man had very ample experimental material at hand on which to base such a conception. In both cases the concept of smallest possible particles was an extremely vague one, so in one sense their roles were very similar, but here the analogy ends. Whereas Democritos's atomistic conception remained alive through the writings of Lucretius, that of Franklin was forgotten for quite a while. In fact, I have no evidence that Franklin's utterances were remembered at all until the time of the emergence of the corpuscular concept. They were not resuscitated until J. J. Thomson dug them out of oblivion in his Silliman lectures in 1903 given at Yale University. Nowhere in the famous papers preceding that of Thomson, dealing with the subject of the electron, could I find any trace of their being aware of Franklin's prediction, and in that sense Franklin's statement was without very much influence on the subsequent development. In the Silliman lectures Thomson, speaking of the determination of e/m, the charge to mass ratio of the elementary charge, says:

> These results lead us to a view of electrification which has a striking resemblance to that of Franklin's "One-Fluid Theory of Electricity." Instead of taking, as Franklin did, the electric fluid to be positive electricity we take it to be negative. The "electric fluid" of Franklin corresponds to an assemblage of corpuscles, negative electrification being a collection of these corpuscles.

One more remark about the use of the word "particles" in Franklin's famous statement. At the time of Franklin the atomistic concept of matter was already quite well advanced. In fact, at least a century earlier the famous natural philosophers had conceived of heat in terms of what we would regard nowadays as a kinetic theory. Bacon, Boyle, and Hooke professed surprisingly modern views about the nature of the heat. Take, for instance, Hooke in his *Micrographia*: "Heat is a property of a body arising from the motion or agitation of its parts." Or listen to Boyle: "Nature of heat" consists in "a various, vehement, and intestine commotion of the parts among themselves." It is hardly surprising, therefore, that, when matter has been considered as being constituted of small particles, Franklin should borrow the expression "particles" for a supposed fluid. We must remember that Franklin's fluid was something material and tangible. Another interesting parallel is that, like Franklin's views that were forgotten for so long, the kinetic views of Bacon, Boyle, Hooke, and others were also forgotten for quite a while. Take this quotation from Whittaker's remarks in his *History of the theories of aether and electricity*:

> Perhaps nothing in the history of natural philosophy is more amazing that the vicissitudes of the theory of heat. The true hypothesis, after having met with general acceptance throughout a century, and having been approved by a succession of illustrious men, was deliberately abandoned by their successors in favor of a conception utterly false, and, in some of its developments, grotesque and absurd.

Franklin's 1-fluid theory found slowly a universal acceptance. It became a good working theory for Faraday, who created the next important step in the theory. In January 1834 Faraday wrote as follows:

> The theory of definite electrolytical or electrochemical action appears to me to touch immediately upon the *absolute quantity* of electricity or electric power belonging to different bodies. It is impossible, perhaps, to speak on this point without committing oneself beyond what present facts will sustain; and yet it is equally impossible, and perhaps would be impolitic, not to reason upon the subject. Although we know nothing of what an atom is, yet we cannot resist forming some idea of a small particle, which represents it to the mind; and so we are in equal, if not greater, ignorance of electricity, so as to be unable to say whether it is a particular matter or matters, or mere motion of ordinary matter, or some third kind of power or agent, yet there is an immensity of facts which justify us in believing that the atoms of matter are in some way endowed or associated with electrical powers, to which they owe their most striking qualities, and amongst them their mutual chemical affinity.

There are two important details to be retained from this statement. One is that there is no mention at all of either Franklin or of any other predecessor. The second is that gradual emergence of the notion of the elementary charge. This elementary charge does not yet exist separately from matter.

It is linked to the atom or atoms and exists only in the electrolytic exchange of electricity. There is no prediction about the existence of such an elementary charge in the normal conditions of conductivity.

The first instance of attributing the phenomena of electrodynamics to the agency of moving electric charges is that of Weber. In 1873 Maxwell, writing on the work of his predecessors, said:

> The electromagnetic speculation which was originated by Gauss and carried on by Weber, Riemann, J. M. C. Newmann, Lorenz, etc. is founded on the theory of action at a distance, but depending either directly on the relative velocity for the particles, or on the gradual propagation of something, with a potential or force, from one particle to the other.

This I have quoted from the foreword of the first edition of Maxwell's famous *Treatise on electricity and magnetism*. No wonder that on page 312 of the same volume, in a section dealing with electrolysis, I find the following quotation:

> Suppose, however, that we leap over this difficulty by simply asserting the fact of the constant value of the molecular charge, and that we call this constant molecular charge, for convenience in description, one molecule of electricity.

Eight years later Helmholtz went even further in his Faraday memorial lecture delivered before the Royal Institution of Great Britain. In this year of 1881 Helmholtz formulated his thought as follows:

> Now the most startling result of Faraday's law is perhaps this; if we accept the hypothesis that the elementary substances are composed of atoms, we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions which behave like atoms of electricity.

All these statements taken together could be interpreted as a clear conception of the corpuscular nature of the elementary charge of electricity. I am somewhat skeptical of this view. None of these statements mentions the evidence obtained in gaseous discharges and for that type of evidence we have to go back almost 20 years.

Exactly 100 years ago, in 1859, the German mathematician and physicist Plücker made a rather surprising discovery. He was investigating the discharge of electricity through a rarefied gas. He was in a better position to investigate that type of phenomenon than were any of his predecessors, because his institution had an outstanding technician named Geissler who invented a new pump for producing a better vacuum. The Geissler tubes, as they soon became known, showed surprisingly beautiful colored phenomena in the discharge which took many forms and shapes. Plücker was in the fortunate position of being the first investigator of this behavior. One particular fact emerged: that there exists a kind of rectilineal propagation of part of the electrical discharge, originating on the negative electrode, hence the name "cathode rays." Without attempting any explanation of their nature, Plücker, and later his pupil Hittorff, observed that these rays can be deflected by a magnetic field. Plücker was originally professor of mathematics and was recognized as one of the leading geometers of his time. When he took over the physics chair, he conducted all his physics research in a remarkably unquantitative way. I deliberately use the word unquantitative, instead of qualitative, because one would expect from a leading mathematician an attempt at a somewhat quantitative formulation of the phenomena he is investigating. Plücker did nothing of the kind, and his successor Hittorff followed in the same way. The German physicist Goldstein did quite a bit to investigate these discharge phenomena, and so did the British physicist Crookes. Slowly, however, people started to speculate about the nature of these strange rays and gradually two views emerged. One was that of Crookes, who maintained that in an electrical discharge, particularly in the cathode rays, matter finds itself in what he called a fourth state of aggregation—a state of aggregation beyond the known three: solid, liquid, and gaseous. The German school of thought was somewhat opposed to this view. Based upon the experiments of Hertz and his pupil Lenard, the view became preponderant on the eastern side of the Rhine that cathode rays consist of some kind of an ether vibration. Lenard found that cathode rays can penetrate through a thin film of aluminum, or other thin material, and can even enter the outer atmosphere. No cor-

puscle, it was thought, would be able to penetrate through dense matter like this, and that is why the electromagnetic wave explanation was preferred by the Germans. Apparently nobody thought that this strange substance could be electricity in free flight, and nobody tried to identify it with electrolytic phenomena or with the conduction of electricity in matter.

The naming day of the electron did not occur until 1874, or 1881, depending on which year is taken as the proper date. In 1874, before the Belfast meeting of the British Association, J. Johnstone Stoney read a paper in which he proposed that the elementary charge of electricity be called the electron. His paper, however, was not published until 1881. Stoney's proposal was a reasonably logical one. Faraday used the word ion quite often in his electrolytical and electrochemical investigations, and ion was accepted for quite a long time. But the noun electron was not derived from the ion; it was derived from the Greek word for amber, from which the word electricity itself has been derived. It was, however, responsible for the introduction of the ending t-r-o-n in our language. This fateful event did not occur until the first or second decade of our century when the magnetron and kenotron were so named. From then on, all kinds of devices were given names ending in "tron," through the somewhat mistaken idea that t-r-o-n means something. I should have preferred that this ending be reserved for elementary particles like the positron and the meson, which was originally called mesotron. However, as the "tron" ending has now been taken over by all kinds of gadgets, it may be just as well that the mesotron has been renamed meson and cannot be confused with the perhapsatron and the swindeltron. Still, I sometimes wonder what some people may think of the electron who are familiar only with the various complicated instruments ending in "tron." Perhaps they think of the electron too as a complicated gadget.

Another remark may be pertinent at this point. At the beginning of the century, particularly in European countries, the word electronics was used to describe the physics of the free electron. Here is another sad instance of the corruption of a word that was reasonably straight-forward and reasonably short. Nowadays, particularly in this country, electronics is completely devoted to the description of high-frequency phenomena, and nobody who wants to speak of the free electron can ever use the word electronics without it being misunderstood.

The real emergence of the modern concept of the elementary charge or elementary particle had to wait until the closing days of the last century. Stoney's electron was still the elementary charge associated with the electrolytic process. The discovery of the photoelectric effect in 1888 by Heinrich Hertz, and its later further development by Hallwachs, did not contribute in any manner to the identification of the gaseous ion with the electrolytic one. This great event did not occur until 1895–1897. Those years saw three very essential steps. First was Perrin's identification of the charge carried by cathode rays with the negative charge. The next extremely important step was the theoretical work of Lorentz, who in 1897 interpreted the then quite new observations of Zeeman and showed that their only possible interpretation was in terms of the action of electrons associated with the atoms. Lorentz also calculated the famous equation of Lorentz force. Later in the same year came the extremely important work of J. J. Thomson, who determined quantitatively for the first time the deflection of an electron beam and determined in this manner the ratio of the charge to the mass of the elementary particles carrying the elementary quantity of electricity. These were the essential steps necessary for formulation of the modern concept, and of the three I should be inclined to say that Lorentz's contribution was perhaps the greatest, although I do not want to belittle Thomson's and Perrin's contributions either. The early part of the twentieth century saw determination of the elementary charge by Millikan. This achievement, however, did not change the conceptual formulation which by that time had been acquired.

The word electron did not find immediate acceptance, and even as late as 1906, the date of the second edition of Thomson's famous book *Conduction of electricity*

through gases, the word never appears. In that book Thomson always calls the elementary charge negative ion. Even slower was the acceptance in certain circles of the notion of the elementary charge, and to many the famous struggle waged by Ehrenhaft against Millikan and many others remains still vivid. Ehrenhaft was convinced that smaller charges than those measured by Millikan and others exist, and he put up a long, hard fight to prove that subelectrons can exist. Ehrenhaft, although somewhat a strange character, was a very good physicist, and he devised many beautiful experiments in an effort to prove the existence of subelectrons. He was very much hurt when his own students became convinced of the futility of his views. He was too stubborn to recognize it, and stories are still going around about his complaint that he has sent one of his best students to California to convince Millikan and instead of that his best student was corrupted by Millikan. I have to thank Dr. DuMond for an anecdote illustrating this controversy. One day Millikan was asked by a lady: "Dr. Millikan, do you spend your *whole time* proving that electricity consists of electrons?" He answered with a smile that thanks to Dr. Ehrenhaft there was getting to be more truth than pleasantry in that question.

When I said that the twentieth century saw essentially the consolidation of the conception of elementary particles, I did not mean to imply that nothing has happened. Since the turn of the century several very important investigations occurred which should be mentioned here. One was the discovery by Kaufmann that the mass of the electron varies with its velocity. This became a splendid confirmation of the special relativity theory. The second important series of investigations demonstrated the inert mass of the electron through the so-called gyromagnetic effects. Four such effects have been listed in the literature. One was Maxwell's proposed experiment, which would show that a magnet, rotated around any axis not coinciding with the direction of magnetization, must act as a gyroscope. The second is the Barnett effect of magnetization by rotation. The famous Einstein–de Haas effect is third: it consists of rotation by magnetization. And last is gyroscopic magnetization by the rotation of a magnetic field, investigated by Fisher and by Barnett.

The last important new feature of twentieth-century work which can be derived essentially from Lorentz's equation of force is known nowadays under the generic term of electron optics. As it is impossible to discuss electron optics without referring at least briefly to the dual nature of the electron, it may be better at that point to close the particle chapter and start the wave chapter.

In concluding my remarks about the corpuscular aspects of the electron, I should like to give a few short quotations. One is drawn from Rosenfeld's book *Theory of electrons:*

> When Thomson's estimate of the specific charge of the cathodic corpuscles became available, no doubt was left as to the identity of Lorentz oscillators with electrons in harmony with Thomson's atomic model.

My other quotation comes from Weisskopf, a review called *Recent Developments in the theory of electron*, which was published in April 1949 in the Reviews of Modern Physics:

> There was hardly any other discovery which left the understanding of so many and varied phenomena as the discovery of the electron... It was mainly H. A. Lorentz who brought the classical electron theory into a consistent frame. These were his fundamental assumptions: the electron is an elementary particle with a charge e and a mass m; the motion of the electrons is determined by classical mechanics if the force acting on the electron is given by the expression:
>
> $$\vec{F} = e\vec{E} + (e/c)(v \times \vec{H})$$
>
> where e and v are the charge and velocity of the electron and $\vec{E}$ and $\vec{H}$ are the electric and magnetic field strengths. The electromagnetic field in turn is given by the Maxwell equations.

And last but not least a few words from Albert Einstein:

> No longer ... do physicists of the younger generation fully realize, as a rule, the determinant part which H. A. Lorentz played in the formation of the basic principles of theoretical physics. The reason for this curious fact is that they have absorbed Lorentz' fundamental ideas so completely that they are hardly able to realize to the full the boldness of these ideas, and the simplification

which they brought into the foundations of the science of physics.

I should like to return now to the tabulation shown at the beginning of this presentation. As we know now, the chief characteristic of the electron, or for that matter of any elementary particle, is the duality of its nature. Depending upon the experiment we carry out, it can behave either as a corpuscle or as a wave. It is logical now that we consider the wave properties of the electron. I briefly mentioned earlier that the late nineteenth century saw a clear division of opinion about the nature of the electron between physicists east of the Rhine and west of the Rhine. In that respect it is interesting to quote the foremost representative of the western opinion. In his classical paper in 1897, J. J. Thomson wrote as follows:

> The most diverse opinions are held as to these rays; according to the almost unanimous opinion of German physicists they are due to some process in the aether to which—inasmuch as in a uniform magnetic field their course is circular and not rectilinear—no phenomenon hitherto observed is analogous: another view of these rays is that, so far from being wholly aetherial, they are in fact wholly material, and that they mark the paths of particles of matter charged with negative electricity.

Yet Thomson's investigations disposed of the wave nature of the electron only temporarily. In 1924, in his famous thesis, de Broglie reintroduced the wave concept in the fundamental properties of the electron. This wave, however, is something different from that of Hertz and of Lenard. The early wave was a truly electromagnetic phenomenon. De Broglie's wave is a guide wave of the particle, not electromagnetic in nature; it is always linked with a particle, it cannot be dissociated from it. The question then may legitimately arise: who were the predecessors of this conception and how was it born? As usual we do find ancestors of the idea, as they are most generously acknowledged in the same thesis of de Broglie. Part of the concept goes back to Einstein. Einstein in his famous paper of 1905 introduced the concept of the dual nature of light where light behaves both as a particle and as a wave. The "needle radiation" concept of Einstein was hard to accept for his contemporaries. Nevertheless, it paved the way for de Broglie, who acknowledges it handsomely. But this is only part of the picture. The more important part of it is that two important fundamental principles of physics—that of Fermat and that of Hamilton–Maupertuis—show more than formal resemblance. On page 24 of his thesis, de Broglie says:

> Guided by the idea of a profound identity of the principle of least action and that of Fermat, from the beginnings of my researches on this subject I was led to admit that,... the dynamically possible trajectories of a moving particle coincide with the possible rays of its wave.

He is even more positive on page 35:

> Fermat's principle, applied to the phase-wave, is identical with the principle of Maupertuis, applied to the moving particle.

The conclusion is simple:

> The preceding results... establish a link between the movement of a moving particle and the propagation of a wave.

The experimental verification was not long in coming. The wonderful work of Davisson, Germer, and of G. P. Thomson is well known to everyone and does not need to be repeated here. What needs perhaps to be repeated is the role Elsasser played in these investigations. Elsasser was, at the time of de Broglie's thesis, a young research student, and he was the first one who found that there may be some experimental evidence in the work of Davisson and Kunsman for the physical existence of de Broglie's waves. In a manner of speaking, this observation by Elsasser stimulated Davisson and Germer to their now classical results. I deliberately used the words "in a manner of speaking" because Davisson emphasized that the first interpretation of Elsasser was not quite right. I am quoting from a note countersigned by Davisson and published in a biography of Davisson by K. K. Darrow.

> The attention of C. J. Davisson was drawn to W. Elsasser's note of 1925, which he did not think much of because he did not believe that Elsasser's theory of his (Davisson's) prior results was valid. This note had no influence on the course of the experiments. What really started the discovery was the well-known accident with the polycrystalline

mass, which suggested that single crystals would exhibit interesting effects. When the decision was made to experiment with the single crystal, it was anticipated that "transparent directions" of the lattice would be discovered. In 1926 Davisson had the good fortune to visit England and attend the meeting of the British Association for the Advancement of Science at Oxford. He took with him some curves relating to the single crystal, and they were surprisingly feeble (surprising how rarely beams had been detected!). He showed them to Born, to Hartree and probably to Blackett; Born called in another Continental physicist (possibly Franck) to view them, and there was much discussion of them. On the whole of the westward transatlantic voyage Davisson spent his time trying to understand Schroedinger's papers, as he then had an inkling (probably derived from the Oxford discussions) that the explanation might reside in them. In the autumn of 1926, Davisson calculated where some of the beams ought to be, looked for them and did not find them. He then laid out a program of thorough search, and on 6 January 1927 got strong beams due to the line-gratings of the surface atoms, as he showed by calculation in the same month.

Nevertheless we have to recognize that Elsasser's ideas were brilliant. He mentioned them at that time to different people, one of them being Einstein. Einstein's answer was: "Young man, you are sitting on a gold mine."

It so happened that the year 1924 was also the year I finished my studies. During the years that followed I saw an extremely rapid change in our picture of the nature of the electron, and with it the whole foundation of physics changed. Let me mention just a few highlights from the five years 1924 through 1928. Following de Broglie's thesis came Schrödinger's famous wave equations. The next year, 1926, saw the introduction of the electron spin concept by Uhlenbeck and Goudsmit. The years 1926 and 1927 saw the birth of electron optics, and 1928 was the year in which Dirac presented his famous theory of the electron. There were so many things happening in those years that one almost forgets that during those same years Heisenberg announced his famous indeterminacy principle.

I should like to examine a little closer one or two aspects of this rapid evolution. First of all, most of these discoveries affected a much greater area of physics than the electron itself. This is in keeping with the older tradition: many years earlier the investigations of J. J. Thomson led into considerations about the structure of the atom. In a similar way the discovery of the wave equation led Schrödinger to a much wider investigation and much more important consequences than the wave behavior of a particle alone.

An interesting example of the application of present-day concepts is the case of electron optics. Electron optics nowadays covers a very wide field. The common interpretation of this expression corresponds to the ray optics of light and covers all phenomena of the interaction of electrons with macroscopic fields of forces. I rather like to distinguish, in close analogy to light optics, between two kinds of electron optics: geometrical and physical electron optics. Geometrical optics considers everything that can be explained with a simple ray concept. Physical optics requires the application of wave principles. In this manner electron optics, in its wider conception, represents a very beautiful example of our present-day concept of the electron at large. At the time of the conception of geometrical electron optics, some beginnings of physical electron optics existed. Scattering had been known for a long time, and electron diffraction was just being discovered. Electron interference was far in the future and so was electron polarization. Geometrical electron optics could very adequately have been described in terms of particle concepts alone. Electron ballistics could be created using the particle concept and all the interactions of the electron in a macroscopic field were properly described in terms of very general equations derived from the Lorentz equation. This description is quite adequate for macroscopic considerations, but it cannot represent the observed phenomena when we reach microscopic dimensions. To explain the resolving power of an electron microscope we must have recourse to the wave concept. This concept also leads to the highly satisfactory give and take which one finds in other branches of physics. The wave concept led us to the discovery of electron interferences and vice versa; the observation of electron interferences gives a more beautiful demonstration of the wave con-

cept than we had before. The recent invention of the electron interferometer at the National Bureau of Standards and its further development in Germany and elsewhere is a good case in point. It shows that very fruitful analogies can be drawn from other branches of physics and applied to the physics of the electron. Here an application of electron interferometer principles taken over from light optics has shown that the optical path difference concept can be extended to electron optics.

Much more could be said about the history of the electron without exhausting the subject. I have not even touched, for instance, such important matters as how Dirac's theory led to the discovery of the positron or Yukawa's theory to the discovery of the meson. Both particles were discovered by Anderson. The meson was originally known under the name of "heavy electron." It is interesting to note that this concept is being resuscitated in a recent note by M. Goldhaber, who suggests that the electron and the μ-meson are two states of the same particle. Here I am attaching a label to the meson, and so we must distinguish between the π- and the μ-mesons. The meson theory of nuclear forces, as we know it today, applies to the π-meson. At the time of its discovery it was thought that the μ-meson would satisfy Yukawa's theory.

These are extremely fruitful and interesting speculations, and one could spend an enormous amount of time on them. My remarks may be summarized in the following fashion: Three names stand out above the others as the chief architects of the present conception of the electron. These are H. A. Lorentz, J. J. Thomson, and Louis de Broglie. In emphasizing their names I do not mean to minimize the contributions of many others. However, I believe that conceptually we owe these three more than we do any other investigators.

This is the end of *my* story, but not that of the electron. After having spent the evening reviewing some phases of the past, I should like to spend one more minute on the future. For me some of the most fascinating aspects of future work on the electron concept lie in the directions indicated by the current fight between the "Copenhagen school" and the "determinists" or "positivists." As is well known, the Copenhagen school's interpretation of quantum events is a statistical one, while the opposing school wishes to ascribe a "physical reality" to the microevent. The arguments used in this fight range all the way from physics to metaphysics; from philosophy to dialectical materialism. Some of the arguments sound like a quotation from Lewis Carroll's *The hunting of the snark:*

> Just the place for a Snark! I have said it twice:
> That alone should encourage the crew.
> Just the place for a Snark!—I have said it thrice:
> What I tell you three times is true.

My hope is, however, that this sometimes acrimonious debate will lead us to a reformulation of the concept which will be acceptable both to the determinist and to the indeterminist.

Wherever modern Science has exploded a superstitious fable or a picturesque error, she has replaced it with a grander and even more poetical truth.—GEORGE PERKINS MARSH

MYCOLOGY.—*Two new species of* Harposporium *parasitic on nematodes.*

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In Petri-plate cultures prepared with decaying plant detritus, eelworms were observed undergoing destruction by two mucedinaceous fungi markedly different from any hyphomycetous parasite hitherto recorded as attacking nematodes. The two fungi are herein described as new species of *Harposporium* Lohde (1874) though neither of them produces conidia of the distinctive crescentic shape signalized in the name of that genus. However, both fungi resemble *H. anguillulae* Lohde emend. Zopf (1888), the widespread species on which the genus was erected, in forming their conidia for the most part on minute slender sterigmata arising from globose cells borne laterally on hyphal elements extended from parasitized animals.

1. **Harposporium baculiforme** sp. nev.

Hyphae assumentes incoloratae, intra vermiculos nematoideos viventes evolutae, simplices vel ramosae, plerumque 2–4.5μ crassae, primo parce septatae sed postea ex magna parte in cellulis 3–20μ longis constantes; genitabiles rami externi, incolorati, clavati, saepius 4–15μ longi, basi 0.6–1.0μ lati, apice 1.5–2.5μ lati, ibi aliquot (fere 2–12) cellulas conidiferas (phialas) ferentes; cellulae conidiferae vulgo globosae sed interdum elongato-ellipsoidae vel obovoideae, 2.5–6μ longae, 2.5–4μ crassae, 1–2 sterigmatibus praeditae; sterigmata vulgo 1–3μ longa, circa 0.6μ crassa; conidia incolorata, baculiformia, aliquando sursum leviter attenuata, basi et apice rotundata, plerumque 2.5–5μ longa, 0.7–1.5μ crassa.

Vermiculos nematoideos interficiens habitat in foliis arborum putrescentibus prope Durango et Steamboat Springs in Colorado.

Assimilative hyphae colorless, growing within living nematodes, in small host animals simple or only sparingly ramified but in larger animals forming moderately branched mycelia, mostly 2 to 4.5μ wide, at first rather scantily septate though later becoming divided into cells mostly 3 to 20μ long, from some of which lateral branches are extended that narrowly perforate the host integument and elongate externally into conidiophores; conidiophores colorless, somewhat club-shaped, 4 to 15μ long, 0.6 to 1.0μ wide at the base, 1.5 to 2.5μ wide at the tip, whereon are borne several (mostly 2 to 12) conidiiferous cells in loosely capitate and sometimes partly in catenulate arrangement; conidiiferous cells commonly globose though sometimes elongate-ellipsoidal or inverted egg-shaped, 2.5 to 4μ wide and 2.5 to 6μ long exclusive of the 1 or 2 sterigmata; sterigmata commonly 1 to 3μ long, about 0.6μ wide; conidia colorless, cylindrical or tapering very slightly toward the apex, always with rounded ends, 2.5 to 5μ long and 0.7 to 1.5μ wide.

Type of species: Figs. 1–14.

Harposporium baculiforme came to light in several maize-meal-agar plate cultures which after being permeated with mycelium of *Pythium ultimum* Trow had been further planted with small quantities of leaf mold collected in woods near Durango in southern Colorado early in July 1958. Later it appeared also in some maize-meal-agar plate cultures that after being overgrown by *Pythium debaryanum* Hesse were further planted with leaf mold gathered in woods near Steamboat Springs in northern Colorado on July 23, 1958. In both sets of cultures it subsisted by parasitizing eelworms referable to a species of *Plectus*. Mostly it attacked relatively young individuals, in each instance extending through the small animal host a single assimilative hypha with scarcely any vegetative branches (Figs. 1–5). Moderate ramification was usual when a more robust animal was invaded (Fig. 6). An assimilative hypha could in some instances be seen to have originated from a conidium lodged in the forward region of the stoma (Figs. 1, a; 5, a). A manner of initiating attack corresponding to that found usual in my *H. bysmatosporum* (Drechsler, 1946, 1954; Aschner and Kohn, 1958) was thus disclosed. Many infected eelworms, however, did not show any recognizable conidium within the stoma, and in these animals the avenue of attack remained conjectural.

The conidiophores of *Harposporium baculiforme* differ markedly in their small dimensions from the robust filamentous conidiophores found in all known congeneric forms as well as in all known nematode-destroying members of the related *Cephalosporium-Verticillium-Acrostalagmus* series. Soon after they have been extended from an infected animal they commonly give rise directly from the slightly expanded tip to globose conidiiferous cells in numbers varying from 1 to 5 (Figs. 1, b, c; 2, a–f; 3, a–g; 4, a, b; 5, b; 6, a–c; 7–11). Some of the globose cells may give rise to 1 or 2 others, so that usually 5 to 12 conidiiferous cells, interspersed with a few sterile cells of similar subspherical shape, become assembled in a loose irregular cluster. Although the globose cells in relatively young clusters most often bear only a single sterigma, many cells in older clusters are provided with two sterigmata (Fig. 11).

If left undisturbed the conidia produced on individual sterigmata tend to cohere side by side in compact sheaves (Fig. 12). Since usually a large proportion of them are moved short distances in different directions by roving eelworms and protozoans they commonly are seen lying separately in haphazard disorder (Figs. 13, 14) around infected eelworms. On careful scrutiny some appear to have a very slightly tapering shape—a spore 1μ wide at its base, for example, diminishing to a width of 0.9μ at its rounded tip.

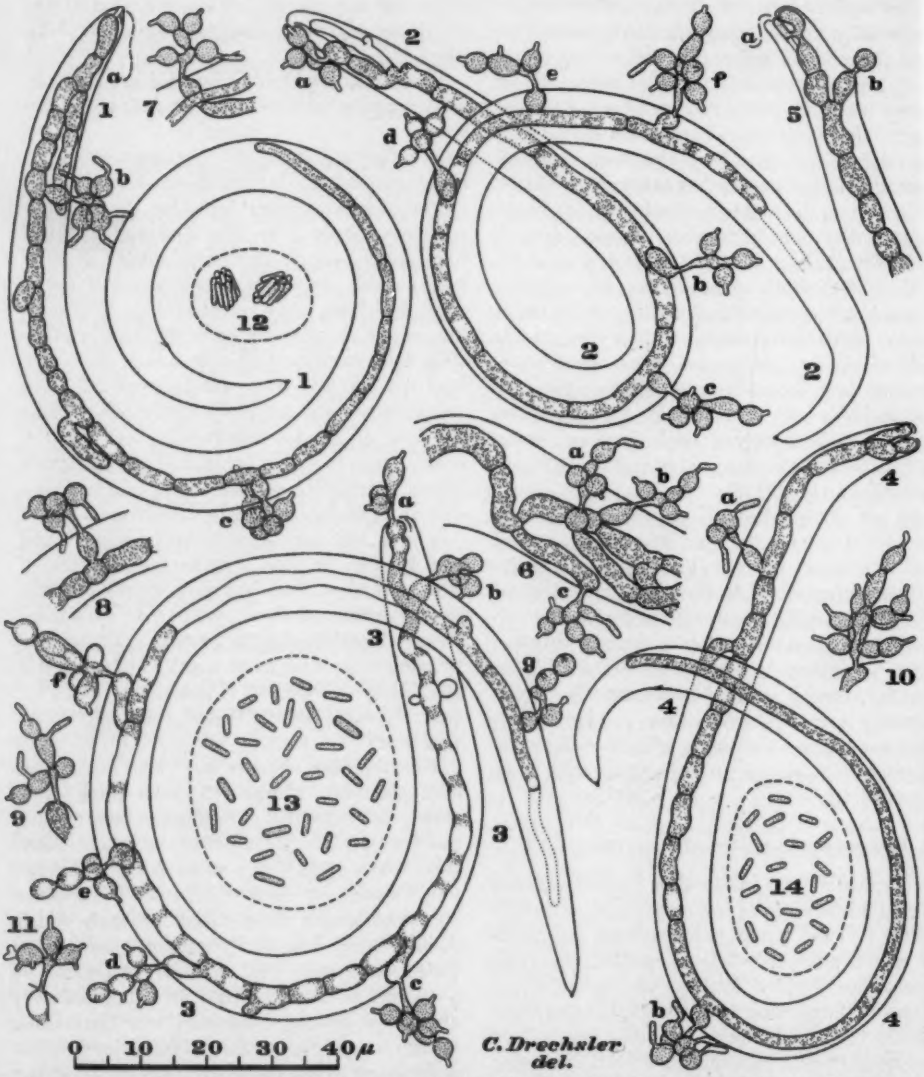
2. **Harposporium sicyodes** sp. nov.

Hyphae steriles incoloratae, intra vermiculos nematoideos viventes evolutae, parce vel mediocriter ramosae, primo parce septatae sed postea in cellulis plerumque 5–20μ longis et 2–4μ crassis constantes; hyphae fertiles extra animal emortuum evolutae, interdum in materia animal ambiente omnino immersae interdum omnino vel ex magna parte procumbentes vel ascendentes, in axe simplices vel ramosae, saepius 10–200μ longae, in cellulis plerumque 4–25μ longis et 2–3.5μ latis constantes, cellula terminalis vulgo conidia ex 1–2 sterigmatibus gignens, aliae cellulae saepius 1–6 ramulos conidiferos ferentes; ramuli conidiferi saepissime globosi sed interdum lageniformes, plerumque 2.8–3.7μ crassi, 1–2 sterigmatibus 1.2–3.7μ longis. et 0.6–0.8μ latis praediti, in toto vulgo 4–8μ longi; conidia incolorata, cylindrata, recta vel leviter curvata, basi et apice rotundata, ita cucumiformia (fructui *Cucumeris sativi* similia), plerumque 3–5μ longa, 0.9–1.2μ lata.

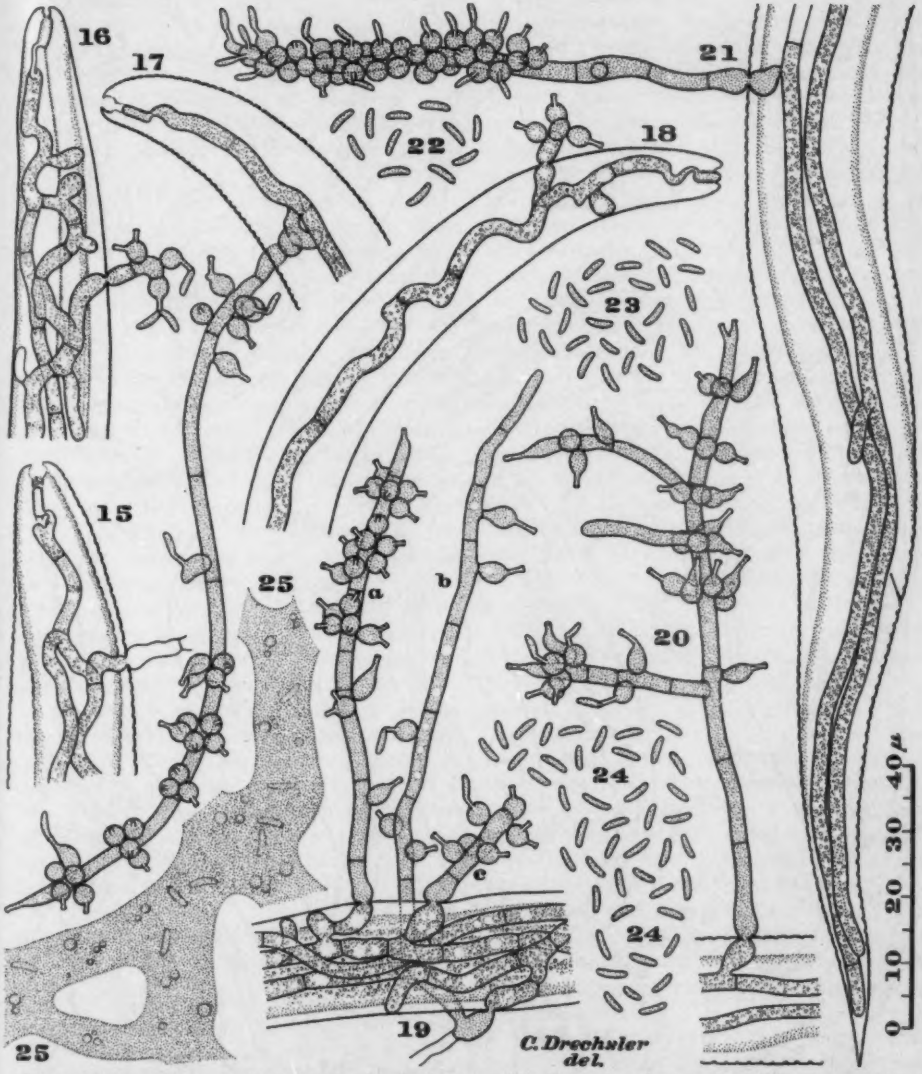
Vermiculos nematoideos interficiens habitat in foliis arborum putrescentibus in Arlington, Virginia.

Assimilative hyphae colorless, growing within living nematodes, in young condition rather sparingly septate, later becoming divided into segments mostly 5 to 20μ long and 2 to 4μ wide; conidiophores developed outside of dead host animal, sometimes submerged and sometimes in varying measure procumbent or ascending, simple or sparingly branched, mostly 10 to 200μ long, composed of cells mostly 4 to 25μ long and 2 to 3.5μ wide—the terminal cell often producing conidia on 1 or 2 sterigmata whereas some or all of the other cells bear 1 to 6 conidiiferous branches (phialides); conidiiferous branch often globose, then 2.8 to 3.7μ in diameter and provided with 1 or 2 sterigmata 1.2 to 3.7μ long, but sometimes variously flask-shaped and 4 to 8μ in total length—the sterigma in either instance often becoming widened at the tip; conidia colorless, somewhat cylindrical though tapering slightly toward both broadly rounded ends, straight or slightly curved, hence resembling cucumber (*Cucumis sativus* L.) fruits in shape, mostly 3 to 5μ long and 0.9 to 1.2μ wide.

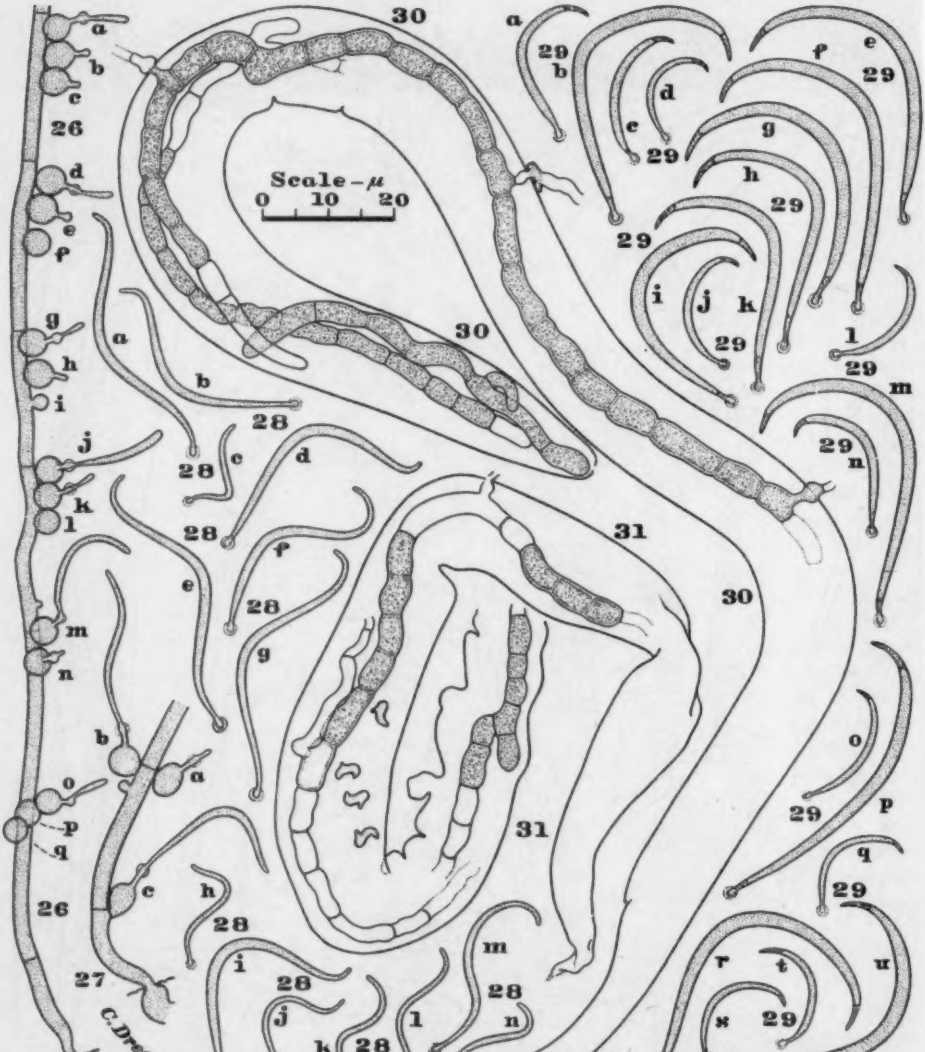
Harposporium sicyodes was found in a maize-meal-agar plate culture that after being overgrown with mycelium of *Pythium vexans* deBary had been further planted with leaf mold taken from woods bordering a watercourse in Arlington, Virginia, on October 11, 1958. It subsisted by parasitizing a sharp-tailed nematode which A. L. Taylor, who kindly examined several infected specimens, held probably referable to a species of *Panagrobelus*. In many infected animals a tubular connection was discernible, though usually with some difficulty, between an assimilative hypha and a conidium lodged in the stoma (Figs. 15–18). Some parasitized eelworms showed one or two ellipsoidal bodies lodged within the oesophagus 20 to 50μ from the anterior end. These bodies may have been swollen infective condia but owing to their deeply imbedded positions the presence of hyphal connections with any assimilative hypha could be neither established nor disproved. Invasion of the eelworm was accompanied often by pronounced withdrawal of the musculature from the



FIGS. 1–14.—*Harposporium baculiforme* from leaf mold gathered in Colorado, partly near Steamboat Springs (1, 4, 5, 8, 13) and partly near Durango (2, 3, 6, 7, 9–12, 14), drawn to a uniform magnification with the aid of a camera lucida, × 1000: 1, eelworm (*Plectus* sp.) with infecting conidium, a, in its stoma, and with 2 conidiophores, a and b; 2, infected eelworm (*Plectus* sp.) with 6 conidiophores, a–f; 3, infected eelworm (*Plectus* sp.) with 7 conidiophores, a–g; 4, infected eelworm (*Plectus* sp.) with 2 conidiophores, a and b; 5, forward portion of eelworm (*Plectus* sp.) showing origin of assimilative hypha from conidium, a, lodged in stoma, and a young conidiophore, b; 6, middle portion of infected eelworm (*Plectus* sp.) with branched assimilative mycelium and 3 conidiophores, a–c; 7–11, conidiophores; 12, conidia cohering in sheaves; 13, 14, two assortments of conidia, each assortment being from a separate infected eelworm.



FIGS. 15–25.—*Harposporium sicyodes* developing parasitically on an eelworm (probably *Panagrobelus* sp.), drawn to a uniform magnification with the aid of a camera lucida, × 1000: 15, forward portion of infected eelworm showing origin of assimilative mycelium from conidium lodged in stoma; 16–18, forward portions of 3 infected eelworms, each showing an assimilative hypha connected with a conidiophore and with a conidium lodged in the stoma; 19, median portion of infected eelworm with 3 conidiophores, a–c; 20, middle portion of infected eelworm with a branched conidiophore; 21, posterior portion of a large eelworm showing a simple conidiophore; 22–24, three assortments of conidia, each assortment being taken from a separate eelworm; 25, eight conidia ingested by a proteomyxan rhizopod (probably *Leptomyxa reticulata*).



FIGS. 26, 27.—Proximal portions of two conidiophores of *Harposporium helicoides* showing, respectively, 17 conidiiferous cells, a–q, and 3 conidiiferous cells, a–c, at different stages of development, × 1,000. FIG. 28.—Detached conidia of *H. helicoides*, a–n, × 1,000. FIG. 29.—Detached conidia of *H. oxycoracum*, a–u, × 1000. FIG. 30.—Indurated hyphae of *H. diceraeum* within integument of an eelworm (*Plectus* sp.) in a culture 65 days old, × 1,000. FIG. 31. Portions of indurated hyphae of *H. diceraeum* that have given rise within integument of an eelworm to 4 new conidia about 45 days after production of conidia on external conidiophores had ceased in the 65-day-old Petri-plate culture, × 1,000.

integument (Figs. 15, 16, 19, 20, 21). Development of assimilative hyphae, especially in posterior regions of large eelworm hosts (Fig. 21), appeared rather less abundant than in nematodes attacked by most related parasites.

The conidiophores of *Harposporium sicyodes*, like those of all congeneric species other than *H. baculiforme*, are moderately stout filamentous hyphae whether they are short (Figs. 16; 18; 19, c) or long (Figs. 17; 19, a, b; 20; 21). They usually remain simple, yet axial branching (Fig. 20) is not exceptional among them. Their unicellular conidiiferous branches, or phialides, are mostly shaped like a Florence flask, with the globose main part being abruptly distinct from the narrow sterigma arising from it. In more than a few instances, however, the distended main part tapers upward and merges with a distal sterigma, so that the conidiiferous cell appears transitional to the type of phialide familiar in species of *Cephalosporium* and *Verticillium*. The slight distension often noticeable at the tip of a sterigma would seem to come about in the abscission of the first conidium.

Harposporium sicyodes, like nearly all of its known congeners, conveniently continues to produce conidia while exposed to microscopical examination in an agar slab under a cover glass. Assortments of its conidia formed on conidiophores extended from separate individual animals (Figs. 22, 23, 24) show only moderate variability in shape and size. In my culture a large proportion of the detached spores were being constantly ingested by a proteomyxan rhizopod (Fig. 25) provisionally identified as *Leptomyxa reticulata* Goodey (1914).

SUPPLEMENTARY OBSERVATIONS ON THREE OTHER SPECIES OF HARPOSPORIUM

Several maize-meal-agar plate cultures which had been planted with leaf mold of the same collection that yielded *Harposporium sicyodes* permitted abundant development of *H. helicoides* Drechsler (1941). Occasion was taken to study more closely the manner in which the phialides of the latter species give rise to conidia (Figs. 26, a–q; 27, a–c). The individual phialide originates as a wart-like protuberance (Fig. 26, i) that continues to grow until it reaches a diameter of 3.5 to 4.5μ (Fig. 26, f, l, p, q). It then puts forth a sterigma usually 0.7 or 0.8μ wide (Fig. 26, c, h), which soon appears to swell distally in forming a terminal globular knob often about 1.5μ in width (Fig. 26, b, e). From this globular knob is now extended a slender outgrowth, approximately 0.5μ wide, whose tubular membrane is clearly continuous not with the globular contour but with a somewhat narrowing tube passing centrally through the knob (Figs. 26, a, d, g, k, n, o; 27, a). The outgrowth elongates first with gradually increasing and later with gradually diminishing width, at the same time describing a helicoid spiral of left-handed rotation (Figs. 26, j, m; 27, b, c). Ultimately the helicoid filament is cut off by a cross-wall at the lower boundary of the knob and then readily becomes detached as a conidium (Fig. 28, a–n). Manifestly the peripheral substance of the knob is of plastic consistency, since in detached spores it covers the basal membrane, and thus persists as a minute deposit of mucus that clothes the very slightly widening proximal end. In all cultures the conidia of *H. helicoides* are given to pronounced variations with respect to size, some measuring as much as 48μ in length and 1.7μ in greatest width, while others have corresponding dimensions of only 20μ and 0.7μ, respectively.

The conidia of *Harposporium oxycoracum* Drechsler (1941) resemble those of *H. helicoides* in having the slightly expanded basal end enveloped in a small quantity of mucus. They often show considerably greater dimensions than were assigned to them in the original account of the species. Thus, among the conidia produced by the fungus (Fig. 29, a–u) in Petri plate cultures that had been planted with forest detritus collected near Beltsville, Maryland, in April 1958, some measured no less than 60μ in total length and fully 2.3μ in greatest width. Moreover, in many of the larger conidia not only the tip but also a proximal portion, usually 2 to 5μ in length, appeared to be filled solidly with wall material.

The lot of forest detritus from southern Colorado that yielded *Harposporium baculiforme* supplied also some development of *H. diceraeum* Drechsler (1941) in several Petri plate cultures. Destruction of the nematode (*Plectus* sp.) parasitized by *H. diceraeum* was apparently completed about 20 days after the cultures had been prepared. When the cultures were 65 days old all external conidiophores of the fungus, together with all the conidia they had borne, had disappeared, but within the integuments of many parasitized animals could be seen variable por-

tions of somewhat indurated living mycelium (Fig. 30). Some of the integuments loosely inclosed a few living conidia of *H. diceraeum* (Fig. 31), which in each instance must have been recently formed on a small conidiophore extended from a single indurated segment. As the hyphal segments here had undergone only slight thickening of their walls and showed only faint yellowish coloration they seemed less strongly modified for endurance than the chlamydospores always produced within eelworms destroyed by *H. anguillulae*.

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GRAPHICAL DIAGNOSIS OF INTERLABORATORY TESTS

A simple way to analyze the discrepancies between different testing laboratories that presumably use the same test procedure was recently worked out at the National Bureau of Standards.[1] Devised by W. J. Youden of the NBS applied mathematics laboratory, the method employs a graphical presentation of the test data which allows each laboratory to tell at a glance how its performance compares with that of others. The graph can point the way to corrective action to eliminate the bias, if any, in the technique used by a particular laboratory; or it may indicate the need for an improved test procedure—one that lends itself better to uniform application by all laboratories. In addition, it provides an estimate of the precision of the test-procedure results.

DISCREPANCIES, NORMAL AND ABNORMAL

Duplication of tests by two or more laboratories is constantly being undertaken in science and industry in order to verify results, to detect systematic errors, and in general to monitor the techniques of measurement. Sooner or later all important results in the physical sciences are checked by other laboratories. In industry, the same quality-control tests may be used by the various plants of a single company and—perhaps alternatively—the same acceptance tests are performed by laboratories at different depots of the purchaser.

In all such cases, discrepancies in the results from different laboratories are not only expected, but inevitable. It is basic to all measurement processes (except those of the crudest sort or those involving only simple counting) that when the same procedure is repeatedly carried out on the same specimen with the same equipment and personnel, the results are not all identical but are scattered over a certain range of values. When the same measurements are made by a number of different laboratories, using nominally identical equipment, the scatter is even greater. In any case, the more precise the procedure, the narrower the range of scatter.

However, when the scatter is unusually large or when a particular laboratory differs markedly from most of the others, something must be wrong. The problem—which the present method of analysis is intended to help solve—is to determine just where the difficulty lies. The difficulty may be due to a number of factors, some of the most important being (1) intrinsic lack of precision in the procedure; (2) faulty technique in carring out the procedure; (3) ambiguity or vagueness in the formulation of the procedure,

[1] For further technical details see the following articles by W. J. Youden: *Presentation for action,* Ind. and Eng. *Chem.* **50**: 91A. Oct. 1958; *Circumstances alter cases, ibid.* **50**: 77a. Dec. 1958; *What is a measurement? ibid.* **51**: Feb. 1959.

causing differences in the way it is applied by different laboratories; (4) differences in the specimens measured; and (5) simple mistakes, such as misreading a dial.

COORDINATING INDUSTRIAL TESTS

The importance of identifying the factors responsible for the discrepancies is clear enough in scientific research. In industry, undiagnosed errors in the carrying out of quality-control tests and errors intrinsic to the test procedure itself are a severe handicap to efficient production. To allow for testing errors, the usual practice is to aim at a higher quality product than the specifications call for. The larger the test errors, the greater the amount of excess quality needed to insure that a reasonable proportion of the finished items will pass the tests—which are still based on the same specifications. Even a modest improvement in the precision or in the uniformity with which tests are carried out could thus lead to a substantial drop in costs.

Under the stimulus of a high rate of new-product development, much attention has re-

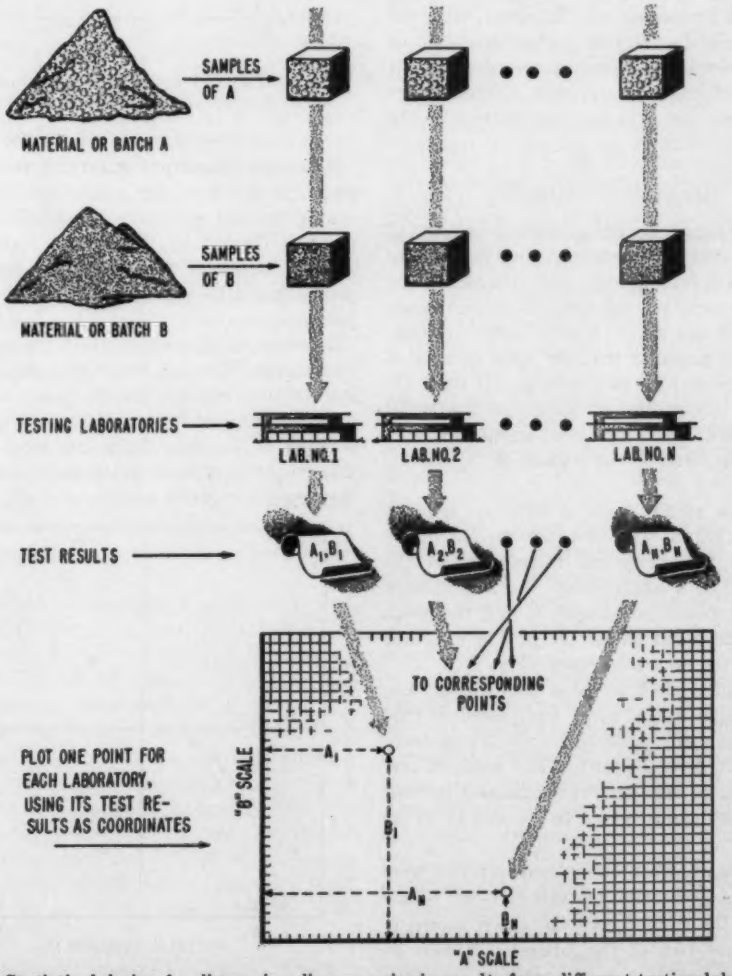


FIG. 1.—Statistical design for diagnosing discrepancies in results from different testing laboratories. The interlaboratory design was developed by the National Bureau of Standards.

cently been given to the problem of coordinating quality-control or acceptance tests in different laboratories. These schemes often use advanced statistical techniques to attack the problem with various degrees of thoroughness, but the practical results have been generally disappointing.

Part of the difficulty seems due to the very sophistication of the methods used. Though modern mathematical techniques have added many powerful tools to the statistical analyst's arsenal, their use demands considerable skill. In contrast, experience with the present method indicates that in a large majority of cases it provides data that "speak for themselves." However, there are circumstances that require a more elaborate or comprehensive interlaboratory test design; but even in such cases the present method might well be used for a preliminary survey of the situation.

THE STATISTICAL DESIGN

In the present statistical design, samples of two fairly similar materials, A and B, are sent to the various laboratories; and each laboratory is asked to carry out the test procedure on each sample. The test results may be single readings or averages, provided that the same number of readings are used for each average. (If there are only a few laboratories, a second pair of samples is distributed later; this is of advantage even when not required by the fewness of the laboratories.)

When the results come in they are used to prepare a graph. The two test measurements from each laboratory are interpreted as the coordinates of a single point; the x-coordinate is the test result on sample A and the y-coordinate the result on sample B. There is thus one point for each laboratory (Fig. 1).

A horizontal median line is then drawn; i.e., a line parallel to the x-axis and placed so that there are as many point above the line as there are below it. A second median line is drawn vertically; i.e., parallel to the y-axis and so that there are as many points to its left as to its right.

For example, when two cement samples were sent to 25 laboratories for tests of 7-day tensile strengths, the results gave the graph shown in Fig. 2. Here two of the laboratories were so definitely separated from the other 23 that they were not used in locating the median lines.

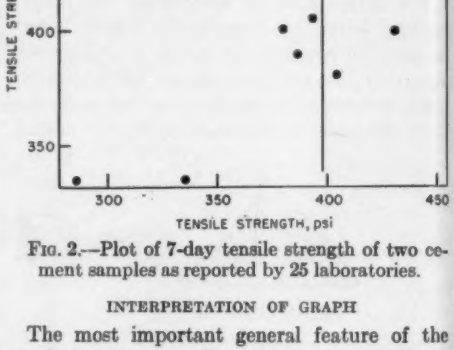


FIG. 2.—Plot of 7-day tensile strength of two cement samples as reported by 25 laboratories.

INTERPRETATION OF GRAPH

The most important general feature of the graph is the way the points are distributed among the four quadrants into which the graph paper is divided by the median lines. If only random errors of precision were present the points should be equally numerous in all quadrants.

However, in all cases to which the method has been applied thus far, there has been a more or less definite tendency for the points to concentrate in the upper right and lower left quadrants. In view of the way the points were obtained, this means that some laboratories tend to get high results on both samples and other labora-

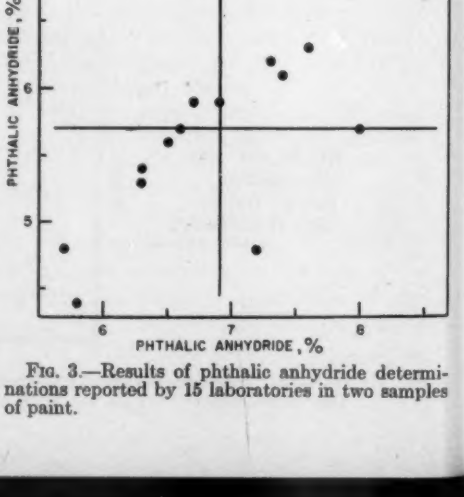


FIG. 3.—Results of phthalic anhydride determinations reported by 15 laboratories in two samples of paint.

tories get low results on both samples. That is, it indicates the presence of individual laboratory biases. Evidence of this state of affairs is seen, for example, in the cement tests (Fig. 2).

The more pronounced the individual laboratory biases, the greater the departure from uniform, circular distribution of the points about the intersection of the medians. Fig. 3 illustrates a case in which the points (with a few exceptions) lie in a long narrow oval with its long axis at about 45° with the x-axis. These points were plotted from phthalic anhydride determinations reported by 15 laboratories on two samples of

Results of the kind shown in Fig. 3 suggest rather clearly that the test procedure needs more careful formulation. In its present form the procedure is apparently open to individual modifications that produce differences in the results obtained. Indeed, the more carefully each laboratory follows its own interpretation of the test procedure, the more closely the points will cluster along the 45° line.

DEVIANT LABORATORIES

Points that deviate far from the intersection of the medians tend to fall into two categories. Either they are far out but close to one of the median lines or they are far out but close to the 45° line (see, e.g., Figs. 3 and 4).

In the first case, the result is fairly good on one material and rather bad on the other. A single instance of this kind might be due to a simple mistake or blunder; but if the same laboratory obtains similar results in succeeding pairs of materials, carelessness may be the reason.

A quite different explanation applies to a point for out on the 45° line. Here the laboratory in question is doing very consistent, careful work; but it has introduced some modification into the procedure so that the results are all too high (or too low). A thorough check on its procedure to discover the source of the bias is in order.

In the copy or the interlaboratory test report that is sent to a particular laboratory, it might be helpful to circle its point in red. This would save the laboratory the trouble of consulting its files to locate itself and would present a vivid picture of its position in respect to the other laboratories.

When an individual point is exceptionally far from the intersection of the medians it is better not to compress the scale in order to include it in the graft. As was done in the example of the cement tests (Fig. 2), such points should be ignored in locating the median lines.

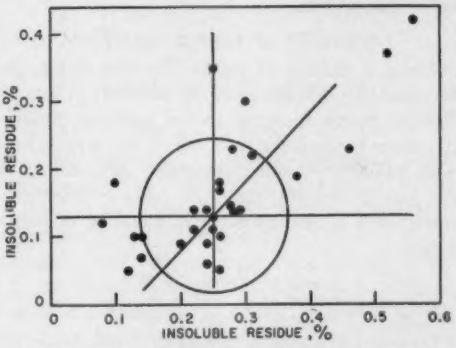


FIG. 4.—Percent insoluble residue in two samples of cement as reported by 29 laboratories. Circle has radius equal to 2.5 times the standard deviation. According to elementary statistical theory, 95 percent or more of the points are expected to fall inside the circle if there are no systematic differences in the techniques used by the different laboratories.

ESTIMATING PRECISION

The graphical presentation of test results, as described above, may also give information on the standard deviation (σ) of a single result. An estimate of σ can be obtained rather simply if it is assumed that (1) the two materials are sufficiently alike so that the dispersion among the results for A should be about the same as that for B; and (2) the differences in precision among the various laboratories are relatively unimportant in comparison to other sources of difference. The first condition can be satisfied by proper choice of the materials and experience shows that as a rule the second condition is then also fulfilled.

One way to get the desired estimate, under the assumptions mentioned, is to find the average distance of the points from the 45° line through the intersection of the medians, and then to multiply this average by $\sqrt{\pi/2}$ or 1.2533. Alternatively, the same estimate can be found by simple numerical calculations from the given results for A and B, without having to measure distances on the graph.

According to elementary statistical theory, a circle centered on the intersection of the medians and having a radius of 2.5 σ should contain 95 percent or more of the points provided there are no constant errors. Laboratories whose points

fall outside such a circle almost certainly are erratic or are following a procedure which deviates substantially from that followed by the other laboratories.

POSSIBILITY OF SAMPLE VARIATION

Could a pattern of points like that of Fig. 2 be caused by differences in the samples? It is not difficult to see that the answer must be in the negative. If the stock from which the A samples were taken were inhomogeneous, and similarly for the B samples, then the pairs of samples distributed to the laboratories would be of four kinds:

high in A, high in B
high in A, low in B
low in A, high in B
low in A, low in B

Since any one of these combinations is as likely to occur as any other, the points representing the test results should be nearly equally divided among the four quadrants. A concentration of points along the 45° line can not be charged to variation among the samples.

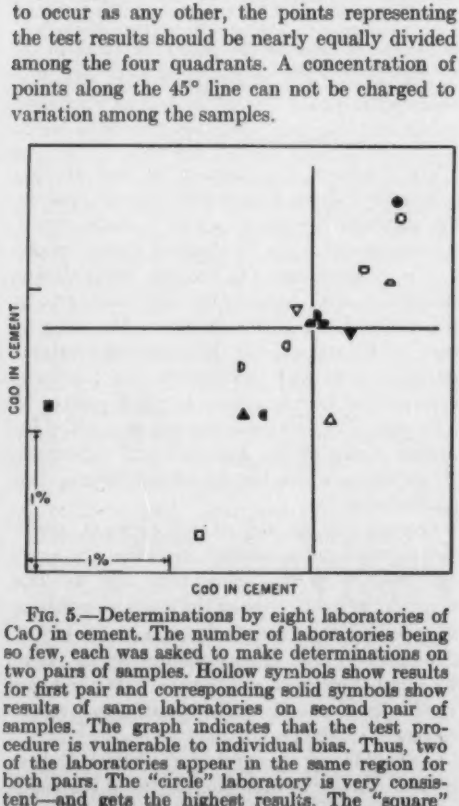


FIG. 5.—Determinations by eight laboratories of CaO in cement. The number of laboratories being so few, each was asked to make determinations on two pairs of samples. Hollow symbols show results for first pair and corresponding solid symbols show results of same laboratories on second pair of samples. The graph indicates that the test procedure is vulnerable to individual bias. Thus, two of the laboratories appear in the same region for both pairs. The "circle" laboratory is very consistent—and gets the highest results. The "square" laboratory gets very low results and is not very precise as shown by the fact that its two points (hollow and solid squares) are farther apart than are the pairs for other laboratories.

However, if there is a roughly circular distribution of points, but with a disappointly wide scatter, the diagram does not reveal whether this arises from sampling difficulties or from poor precision in testing. Further light on this problem can be obtained by a modification in the method of assigning samples. If there are 2N laboratories, N *double-size* samples of each material are prepared. By mixing or otherwise, it should be possible to divide each double-size sample into two closely matching halves. The samples are now distributed in such a way that if the two halves of a particular double sample of A go to laboratories X and Y, then these same laboratories receive the two halves of a double sample of B.

If sample variation is the trouble, the points in the new graph will tend to appear as N closely spaced pairs—like double stars in the heavens. On the other hand, if the points corresponding to the two halves of a double-size sample are separated as much (on the average) as points from different double samples, the scatter cannot be ascribed to sample heterogeneity.

SUMMARY OF ADVANTAGES

The present interlaboratory test scheme requires of each laboratory the relatively light task of measuring only two samples; and the collation of test results involves a minimum of computation. Interpretation of the graph requires no professional statistical background to follow the reasoning; yet it permits a fairly searching examination of the test procedure itself and of the way it has been carried out by the individual laboratories.

Directions for improvement are clearly indicated. A long narrow ellipse directs attention to a more careful description of the procedure or even to the need for its modification. Points far out and near one of the medians indicate erratic work; points far out along the 45° line are strong evidence of deviations from specified procedure. Use of a circle with a radius of 2.5 σ shows the individual laboratory whether or not its technique has in some way become saddled with a substantial constant error. Experience has already shown that a certain few laboratories turn up all too frequently in the most distant positions from the intersection of the medians. Improved performance from those few laboratories may go far to restore confidence in a test procedure.

ENTOMOLOGY.—*A new grasshopper of the genus* Achurum *from eastern Texas (Orthoptera: Acrididae)*. ASHLEY B. GURNEY, Entomology Research Division, United States Department of Agriculture.

(Recieved February 18, 1959)

One of the most distinctive genera of North American grasshoppers is *Achurum* Saussure,[1] characterized by a very slender and elongate body and an extremely oblique, or ventrally "undercut," face. An individual clinging parallel to a grass stem with its antennae extended forward would scarcely be distinguishable from the vegetation. Since the publication of Hebard's review of the genus (Trans. Amer. Ent. Soc. **48**: 89–93. 1922), the genotype, *A. sumichrasti* (Saussure), has been the only included species. It was shown in habitus illustration by Ball et al. (Univ. Arizona Techn. Bull. 93: 287. 1942). The characters of the genus were also treated by Hebard in his key to Nearctic genera of Acridinae (Trans. Amer. Ent. Soc. **52**: 47–59. 1926). Though of distinctive appearance, specimens of *Achurum* are uncommon, and in the United States *sumichrasti* is known only in southeastern Arizona and southwestern Texas.

For the privilege of studying the new species here described I am very grateful to John R. Hilliard, of the staff of McMurry College, Abilene, Tex., who collected all the material, and to whom I am glad to dedicate the species in recognition of his unpublished studies of grasshoppers and in appreciation of his cooperation. I am also grateful to J. A. G. Rehn, Academy of Natural Sciences of Philadelphia, for the loan of specimens of *sumichrasti* needed for comparative purposes to supplement specimens in the U. S. National Museum.

The new species averages larger than *sumichrasti*, and it is separated by differences which in the main are subtle rather than striking. The most noticeable differences are that the male subgenital plate of *hillardi* is more elongate and acute and the fastigium of the vertex has wider marginal areas.

[1] Saussure (Rev. Mag. Zool. 1861: 313) adapted the name *Achurum* from the "Greek" achyron, meaning chaff, husk, or scale.

Achurum hilliardi, n. sp.

Figs. 1, 2, 6, 8, 9, 11, 13

Male (holotype): General appearance much as in *sumichrasti*; fully winged. Apex of fastigium in dorsal view angulate rather than evenly rounded (usually but not always true of *sumichrasti*), and with marginal areas laterad of impressed longitudinal grooves (Figs. 5, 6, *lg*) wider than in *sumichrasti*, in lateral view with lateral margins decurved at apex (Fig. 1, *lm*) and with portion of frontal costa (*fc*) anterior to antennal bases obtuse-angulate rather than more broadly rounded as usual in *sumichrasti*.

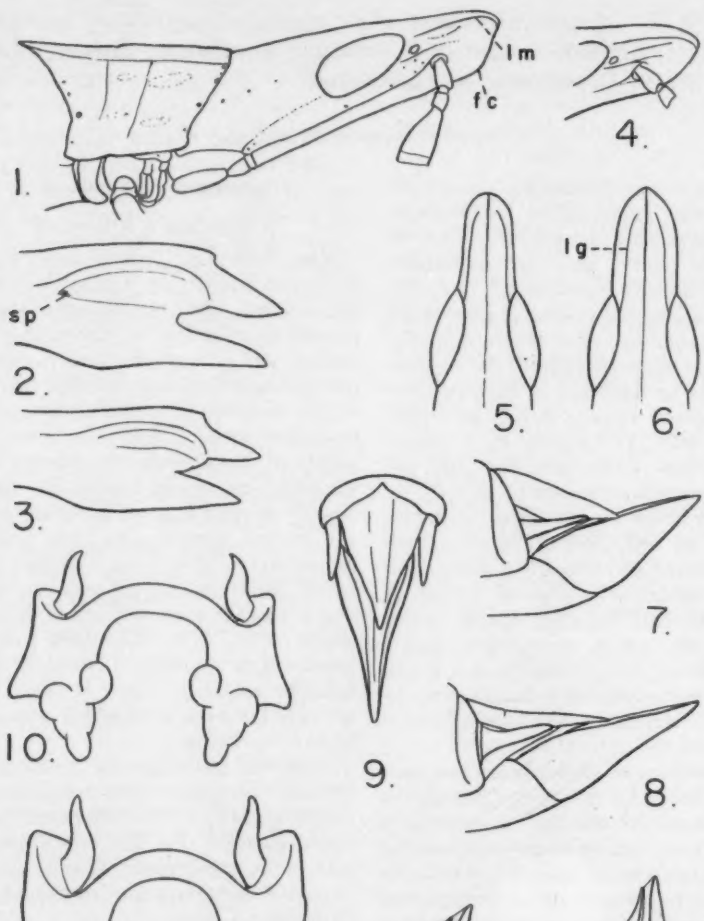
Pronotum, tegmina, and wings essentially as in *sumichrasti*; hind femur with both mesal and lateral dorsal and ventral genicular lobes produced toothlike somewhat longer than in *sumichrasti* (Figs. 2, 3); hind tibia with variable number of spines (left: 17 lateral, 22 mesal, one apical on mesal only; right: 16 lateral, 19 mesal, one apical on each side), apical spurs similar to those of *sumichrasti*.

Subgenital plate apically produced, not essentially triangularly acute in lateral view as in *sumichrasti* (Figs. 7, 8); supra-anal plate (epiproct) elongate (Fig. 9); cercus slender, elongate, as in *sumichrasti*; (phallic complex not extracted, described under *variation* below from paratypes).

Coloration: In general pale brown; antennae and compound eyes reddish brown; dorsal surfaces of closed tegmina much paler than lateral surfaces; dorsal surface of pronotum somewhat paler than lateral lobes, latter with conspicuous blackish spot near anterior margin, another spot near posterior margin (Fig. 1); hind femur with tiny dark spots (10 left, 12 right) along the midlongitudinal line of the lateral (outer) paginal area, both mesal and lateral genicular disk with blackish spot at anterior margin (Fig. 2, *sp.*).

Measurements (length in millimeters): Body, 32.2; antenna, 14.0; fastigium anterior to compound eyes, 2.5: pronotum, 4.7; tegmen, 28.0; hind femur, 14.5; greatest width of hind femur, 1.8.

Variation among other specimens: The shape



FIGS. 1–13.—1, *Achurum hilliardi*, n. sp., lateral view of head and prothorax of holotype, only bases of antenna and leg shown; 2, *A. hilliardi*, apical portion of left hind femur of holotype; 3, *A. sumichrasti* (Saussure), same view as Fig. 2, male from Cuernavaca, Mexico; 4, *A. sumichrasti*, laterial view of apical portion of head of male from Baboquivari Mountains, Ariz.; 5, *A. sumichrasti*, dorsal view of anterior portion of head, same specimen as Fig. 4; 6, *A. hilliardi*, same view as Fig. 5, holotype; 7, *A. sumichrasti*, lateral view of apical portion of abdomen, male from Fort Grant, Ariz.; 8, *A. hilliardi*, same view as Fig. 7, holotype; 9, *A. hilliardi*, dorsal view of apex of abdomen, holotype; 10, *A. sumichrasti*, dorsal view of epiphallus, KOH preparation, male from Baboquivari Mountains, Ariz.; 11, *A. hilliardi*, same view as Fig. 10, paratype; 12, *A. sumichrasti*, lateral view of aedeagus, KOH preparation, same specimen as Fig. 10; 13, *A. hilliardi*, lateral view of aedeagus, KOH preparation, paratype. (*fc*, frontal costa; *lg*, longitudinal groove of fastigium; *lk*, lateral knob of epiphallus; *lm*, lateral margin of fastigium; *mk*, mesal knob of epiphallus; *sp*, dark spot on genicular disk.) (Drawings by author.)

of the fastigium in dorsal view varies somewhat among the paratypes, but in none is it evenly rounded at the apex as is usual in *sumichrasti*. In lateral view the position of the frontal costa anterior to the antennal bases is always decidedly obtuse-angulate, but specimens of *sumichrasti* occasionally are sufficiently angulate so that this feature is not a constant separating character. Spines along the hind tibia vary as follows as regards extremes and averages: Lateral, 13–18, av. 16.1; mesal, 17–21, av. 18.9. In contrast, the tibial spines of 9 males of *sumichrasti* from Texas, Arizona, and Mexico have been examined with the following corresponding results: Lateral, 13–16, av. 14.8; mesal, 16–20, av. 17.7. The apex of the supra-anal plate is weakly sclerotized and consequently the length of the organ varies, but in most specimens an acute apical portion is moderately developed. This organ in *sumichrasti* is very similar, not always as short and blunt as might be inferred from Hebard's description (Trans. Amer. Ent. Soc. **48:** 93. 1922). The female nymphs (not considered paratypes) show a very elongate second portion of the supra-anal plate similar to that described and figured for *sumichrasti* by Hebard.

The phallic complex of four paratypes has been examined and compared with examples of *sumichrasti*. The shapes of the aedeagal valves may differ slightly in the two species (Figs. 12, 13), but individual variation in preparations appears to render separation on this basis uncertain. Figs. 10 and 11 depict epiphalli of *sumichrasti* and *hilliardi*, and the limited dissections made indicate that differences occur in the mesal and lateral knobs of the lophus. The lateral knob of *hilliardi* (Fig. 11, *lk*) is more evenly rounded when seen in lateral view than that of *sumichrasti*, which is rather abruptly terminated at its anterior margin. The mesal knob of *hilliardi* (*mk*) apparently is less produced mesally than that of *sumichrasti*.

The general coloration of the paratypes is comparatively uniform, but two specimens lack dark spots on the lateral paginal area of the hind femur, and one of them has the two dark spots on the lateral lobe of the pronotum obsolete.

Measurements (extremes and averages, in millimeters) of the paratypes are as follows: Body, 29.0–33.0, av. 31.6; antenna, 13.5–15.5, av. 14.1; fastigium anterior to eyes, 2.3–2.6, av. 2.5; pronotum, 4.3–4.9, av. 4.6; tegmen 27.4–30.5, av. 28.7; hind femur, 14.5–15.0, av. 14.8; greatest width of hind femur, 1.6–1.8, av. 1.75.

To show the average larger size of *hilliardi*, 14 males of *sumichrasti* from localities throughout the latter's range have been measured for three dimensions, with results as follows: Pronotum, 3.6–4.6, av. 4.1; tegmen, 22.0–28.0, av. 25.8; hind femur, 11.5–14.0, av. 13.1. It should be noted that tegmen length has been measured "from the distal extremity of the tubercle formed by the junction of the subcostal and radial veins" to the extreme tip. (Proc. 4th Internat. Locust Conference, Cairo: 97. 1937). Dirsh (Anti-Locust Bull. 16, fig. 1. 1953) illustrated this dimension.

Type: U. S. National Museum no. 61125.

Type locality: A bog 2 miles south of Warren, Tyler Company, Tex.

The holotype male, 8 male paratypes, and 3 females apparently in the instar preceding maturity bear identical data, having been taken at the type locality April 24, 1955, by J. R. Hilliard. Paratypes will be deposited in the collection of Mr. Hilliard, and those of the Academy of Natural Sciences of Philadelphia, the University of Michigan, and the U. S. National Museum.

Mr. Hilliard has furnished the following ecological notes:

> The site is located just off U. S. Highway 287/69. The lumber road leading to the collection site turns off the main highway 2 miles south of Warren, Tyler, Tex. The specific area is a low, wet, boggy meadow just across the Texas and New Orleans Railroad tracks and includes the railroad right of way. The area is an open meadow dotted with crayfish chimneys. The fine, light sandy soil supports a lush growth of sedges, grasses, and numerous clumps of the pitcherplant *Sarracenia sledgei* McFarlane. The adjacent wooded area consists of pine, oak, and magnolia.
>
> This locality is a part of Tharp's region no. 1, the Longleaf Pine Region of Texas (Vegetational Regions of Texas, in *Texas range grasses*, by B. C. Tharp, 1952). According to Tharp's map of the average annual precipitation, which is adapted from *Climate and man* (Yearbook of Agriculture, 1941), the average annual precipitation in this region would be between 48 and 50 inches.

The habitat of *hilliardi* apparently contrasts sharply with that of *sumichrasti*; the latter has seldom been discussed, but so far as recorded it is much different from the boggy environment occupied by *hilliardi*. Hebard (1. c.: 93) stated that Rehn captured a single specimen while beating bear grass (*Nolina* sp.) in the Sierritas

Mountains, Ariz. Ball (Journ. Econ. Ent. **29:** 680. 1936) said that it feeds on broom rape (*Andropogon barbinodis* Lag.), and in 1942 (Ball et al., 1. c.: 286) stated that it is found on "coarse grasses on rocky slopes in desert grassland of the Lower and Upper Sonoran zones of southeastern Arizona." In the Chinati Mountains near Shafter, Tex., Tinkham (Amer. Midl. Nat. **40:** 565, 574. 1948) found a localized colony in tall grass at the bottom of a steep cut on the slope of a high plateau at about 5,500 feet elevation, and his figure 24 is a photograph of the habitat.

The distribution of *sumichrasti* was stated by Ball et al. (1. c.: 286) as "Southeastern Arizona west to the Baboquivari and Quinlan Mountains; north to the Catalina and Pinaleno Mountains . . . southwestern Texas, and south to Mexico and Guatemala." Texas records are limited to Jeff Davis, Brewster, and Presidio Counties. Specimens from El Salvador (L. Olomega, Dept. San Miguel) and Nicaragua (Managua) are in the U. S. National Museum, but there is no information about the ecological conditions of the localities where they were taken. Saussure (1. c.: 313) originally described *sumichrasti* from temperate Mexico, and Tinkham (1. c.: 646) regarded it in the United States as belonging to the Mexican Upper Sonoran fauna. Coahuila and Veracruz are nearer to eastern Texas than any other Mexican areas from which *sumichrasti* has been recorded.

BIRD MIGRATION STUDIES

Some of the familiar present-day mass bird migrations in spring and autumn may stem from habit developed during forced ebb and flow of avian populations in the Pleistocene or Ice Age, which ended 30,000 years or less ago. In some way the migratory instinct seems to have been built into the annual life cycle of the species. This thesis is discussed by Dr. Alexander Wetmore, Smithsonian Institution research associate, in a publication recently issued by the Institution. It appears to be corroborated, he says, by the route patterns of migrations from different parts of North America.

"Among the multitudes of migrants that come south into Central America," Dr. Wetmore says, "eastern and western species mix in abundance as far as southern Guatemala. Farther south, eastern species predominate, and comparatively few of those that nest between the Rocky Mountains and the Pacific coast reach the Isthmus of Panama.

"The ice front of the last glacial period in the eastern half of North America extended south to Long Island, the Ohio Valley, and the Missouri River above St. Louis, with consequent displacement of Temperate Zone conditions far southward. In the west, where the ice front lay a relatively short distance below the United States-Canadian boundary, pressures toward the south would have been far less severe. Eastern species either had to cross the water barrier of the Gulf or to follow the shoreline west and southwest, and so tended to penetrate farther into Central America. Western populations on the other hand had necessity for shifts of less extent."

One interesting migration, Dr. Wetmore says, is that of certain species of vireos. The red-eyed vireo breeds commonly over a wide area from British Columbia to Nova Scotia and south to Texas and central Florida. In the fall this vast group of individuals goes south to winter in the upper basin of the Amazon. A closely related species, the yellow-green vireo, nests in the Tropics from Mexico to Panama. This bird certainly has no need for a seasonal migration because of climate. But it migrates also in September and October to join the redeyes in the upper Amazon. Another, the black-whiskered vireo, nests in southern Florida and the West Indies, where cold of changing seasons is never a problem. But most of these birds also migrate into the same general region as their relatives.

"We may theorize," says Dr. Wetmore, "that there is some tie for this habit back to Pleistocene climatic conditions. This appears clear in the case of the red-eyed vireo, but obscure with the others that now nest in regions that are tropical. It seems possible that during the period of the Wisconsin ice sheet temperature conditions in these areas of the northern Tropics were so different that an annual migration was established which now continues though there is no apparent present necessity for it."

History of the northward surges of bird populations may be repeating itself today, the ornithologist points out. During the past century weather records in the Northern Hemisphere

show a slight but steady advance of warmer temperatures toward the north. The shift seems to have been accelerated in the past 30 years. Slowly birds are following the change that this brings. In Iceland it is reported that due to warmer conditions in the past 50 years, indicated by retreat of glacial ice and the decrease of the ice pack on the northern coast, seven species of birds have been added to the breeding population. These include such diverse forms as the starling, short-eared owl, herring gull, and tufted duck. There also has been an increase in migrant species and in those that remain through the winter.

In Greenland there has been recorded a more interesting instance. Twenty years ago there was a chance invasion of a flock of fieldfares, an Old World thrush that nests in birch and coniferous forests of northern Europe and northwestern Siberia, ranging north nearly to the tree line. Wanderers remained in southern Greenland to establish a breeding colony, thus populating a land area quite remote from their previous range. Some Greenland birds migrate in fall to return in spring, but the fieldfare, although a migrant in its proper home in northern Eurasia, remains a resident in southern Greenland.

EXCAVATIONS AT LA VENTA

On an inland island, a dry land area of about 2 square miles surrounded by a jungle swamp in southeastern Mexico, stood, between 2,000 and 3,000 years ago, one of the great religious centers of the aboriginal New World. It consisted of a stone pyramid 103 feet high and with a base of more than 10,000 square feet, a ceremonial court where rites were conducted and sacrifices made to a fantastic jaguar god, and various other structures. This was the La Venta site in the Tabasco province near Veracruz, chief extant monument of the mysterious Olmec people who apparently evolved a civilization roughly contemporaneous with, but quite distinct from, those of the Mayas and Incas to the south. They preceded by many centuries the Aztecs whose culture was dominant in Mexico at the time of the first European explorations.

The La Venta site has been the scene of excavations by archeologists for nearly 20 years. Latest of these was the uncovering of the ceremonial court, just north of the great pyramid, by a cooperative expedition of the Smithsonian Institution, the National Geographic Society, and the University of California, under the direction of Dr. Philip Drucker, Smithsonian archeologist, and Dr. Robert F. Heizer of the University of California. It was a mammoth job, carried out with pick, shovel, and wheelbarrow—for machinery could not be brought into the site—by a labor force of 50 men in 100 days. Helpful advice and assistance were provided by the Instituto Nacional de Antropología e Historia and the Museo Nacional de Antropología of Mexico.

The joint report on this work, by Drs. Drucker, Heizer, and Robert J. Squier, has recently been published by the Smithsonian's Bureau of American Ethnology. The findings throw considerable new light on the ways of life, especially the religious practices, of the Olmec people. Datings by the carbon-14 technique show that artifacts found there covered a range between about 2,300 and 3,000 years ago. This is essentially contemporaneous with the Maya cultures of Yucatán and Guatemala. Still, very little is yet known as to the vanished Olmecs who had disappeared long before the 16th century, essentially "without a history" and leaving almost no clues as to their identity. A little, however, can be reconstructed from the results of these investigations. La Venta, the authors hold, was a ceremonial center operated by a small group of priests, or priest-kings—and their personal servants, supported by tributes from villages in the general vicinity and with large labor forces recruited from these same villages. Even after the original construction, the excavations show, much labor was essential for repair and maintenance.

> The fact that the site was in continuous use for about 400 years is a clear indication of extraordinary cultural stability and singleness of purpose. The Olmec religion must, at the time of the beginning of the site, have already been a well-worked-out system which had sufficient meaning, tradition, and purpose to insure its continuance for nearly half a millennium.
>
> That the society which built and gloried in the La Venta site had an agricultural economy seems quite likely, although we have no direct evidence. No actual remains of maize have been found although the mano and metate, usually associated with maize agriculture, do occur. That this culture group had master artisans who could sculpture basalt, work jade, and polish metallic mirrors is established. But all this tells us little else than the fact that ... some rather elaborate sociopolitical or socioreligious organization was in existence.

ZOOLOGY.—*Taxonomy of the copepod genera* Pherma *and* Pestifer.

R. U. Gooding, University of Washington. (Communicated by Fenner A. Chace, Jr.)

(Received February 2, 1959)

In 1923, C. B. Wilson instituted the genus *Pherma* for a new species of copepod, *P. curticaudatum*, specimens of which had been collected from an annelid off the coast of Lower California. He placed it in the Clausiidae, a family containing a number of other annelid associates. However, in revising this group, M. S. Wilson and Illg (1955: 135) considered that "it would be impossible to assign (*Pherma*) to any family, since C. B. Wilson omitted a description of the oral appendages."

Twenty-one years later, in a paper published posthumously, C. B. Wilson (1944) described another new genus, *Pestifer* (with genotype *P. agilis* also from an annelid but in the Gulf of Mexico), which he referred this time to the Clausidiidae. Again the mouthparts were not discussed.

His descriptions of these two species were sufficiently similar for Wilson and Illg (loc. cit.) to suggest an investigation into their possible congeneracy. Accordingly, advantage was taken of a visit to the United States National Museum in September 1958 to pursue this problem (as part of a taxonomic analysis of the Clausidiidae, on which I am currently engaged) Examination of the type material available there of both Wilson's genera indicated that a single species is represented; this is redescribed.

Pherma curticaudatum C. B. Wilson, 1923

Synonym.—Pestifer agilis C. B. Wilson, 1944: 546–547, pl. 31, figs. 165–171.

Material examined.—The syntypes are "three adult females, one of which bore egg strings ... from the parapodia of an unnamed annelid, dredged from a depth of 645 fms. by the Bureau of Fisheries steamer 'Albatross' off the coast of lower California in April, 1911. These ... are deposited in ... the American Museum of Natural History (Cat. No. 4617)." (Wilson, 1923.) Two specimens—one ovigerous but without maxillipeds, the other lacking the cephalosome and one of each of the three pairs of swimming legs—were found in the U.S. National Museum collections (no. 59354: the label with them also lists the *Albatross* station number as D5685); both were dissected for study. The third is still in the original repository. It was examined without dissection after clearing in methyl salicylate. This specimen lacks only the right maxilliped and caudal rami but unfortunately, in the course of preparation, broke in half.

The other specimens studied were two females, from the skin of an annelid dredged near the Tortugas Islands, Gulf of Mexico, in 380 fathoms, July 18, 1932; USNM no. 79641. Although Wilson (1944) lists one of these as the holotype of *Pestifer agilis*, both were included in the same vial. It is possible that the female which I dissected is the one on which most of Wilson's drawings were based, since it was devoid of one of each pair of the appendages he figured and both maxillipeds. The other was examined as a temporary whole mount in lactic acid; one maxilliped and both ovisacs have been removed during the course of this investigation.

To simplify reference to these specimens, the following abbreviations are used: "Ph. 1," the ovigerous *Pherma* female; "Ph. 2," the mutilated *Pherma* female; "Ph. 3," the *Pherma* female in the American Museum; "Pe. 1," the relatively undamaged *Pestifer* female; and "Pe. 2," the *Pestifer* female which I (and Wilson?) have dissected.

Female.—Wilson's figure of the habitus (1923, fig. **1**)[1] is better than any which could be attempted with the existing material; it shows very well the delimitation of the first pedigerous segment both from the second and from the cephalosome,[2] the remainder of the metasome swollen and fused into a single mass whose three constituent segments are indicated by constrictions at approximately equidistant intervals along the body, and the abrupt narrowing at the origin of the 3-segmented urosome. Lacking, however, is any clear indication that the first segment of the urosome is set off from the last

[1] All references to figure numbers from C. B. Wilson's papers have here been placed in boldface type.

[2] The latter division, as he mentions in the text, is absent on the ventral side.

metasomal segment; a definite line of thickening does in fact occur at this point. The dorsal boundary between the genital and anal segments is also less well defined than shown in his drawings.

Another fact which he does not mention or indicate in his figures is that the genital segment can be seen in ventral view (Fig. 16) to have an irregular thickened line, running transversely just anterior to the openings of the oviducts, which divides it into an anterior and a posterior part. In the latter of these, two dark areas (which may represent paired seminal receptacles) show up very distinctly even in cleared specimens.

The caudal rami (Fig. 9) are neither "jointed" (Wilson, 1944) nor "destitute of setae" (Wilson, 1923). This confusion has evidently arisen because the middle seta of the three terminal ones on each ramus is enlarged nearly to the diameter of the ramus itself. There is a slight transverse constriction in its proximal portion which probably corresponds to the end of the basal "peg" characteristic of one or two of the terminal caudal setae in less modified copepods. The other two setae at the end of each caudal ramus are slender and, in most of the specimens, lie close to the shaft of the main one, so that it is not easy to determine their exact lengths. There are also two short setae on the outer face of the ramus, both in the distal half, and one dorsally—all slender and inconspicuous.

The considerable difference between the lengths which Wilson gives for the body of *curticaudatum* (4.40 mm) and of *agilis* (6.24 mm) is not shown by the specimens. It was difficult to make measurements since the only whole animals curved; rough estimates, for which a ruler and dissecting microscope were used, indicate that Ph. 3 was about 4.5 mm long (without caudal rami) and Pe. 1 more than 3.7 mm. Since the metasome and urosome of Pe. 2 are of comparable size to those of Pe. 1, it is probable that Wilson's first figure is the more accurate.

In all the specimens which possessed a cephalosome there is a groove separating the anterior part of the ventral surface from the wide, nonprotuberant rostrum (Fig. 10). The "ventral cephalic shield" so delimited partially overhangs (in ventral view) the lateral depressions where the antennules and antennae insert, and its border continues thereafter as a thickened ridge, terminating eventually at the extreme posterior corner of each maxilla (Fig. 11).

No postoral protuberance, between the maxillipeds and first pair of legs, could be distinguished.

The antennules are slender structures with six podomeres (Fig. 1). No evidence could be found for the division of the basal podomere into the two short ones shown by Wilson (1944, pl. 31, fig. **167**)—no doubt the reason why he termed this appendage 7-segmented—nor for his claim (loc. cit., p. 547) that "the only setae are terminal on the end segment." The pattern appeared to vary among the specimens; its most complete form (Ph. 1, left) was—proximal to distal podomeres: 2, 6, 3, 2, 2 plus 1 aesthete, and 7 plus 1 aesthete.

The antennae (Fig. 2) may have three or four podomeres, since the division between the third and fourth—a line at best—is sometimes completely absent. The terminal armature accords well with what I consider to be basic among poecilostomes: a row of curved setae (here three instead of the more usual four) between, on the one side, two more slender setae, which are located in a depression behind the tip of the appendage toward its outer face, and one more distally placed on the other; but that on the third podomere is reduced to a single seta, usually accompanied by three small elements. Nothing could be found on the first two podomeres. It is possible that Wilson (1944) partially confused the antennae with the maxillipeds since, in his generic diagnosis of *Pestifer* (p. 546), he speaks of them as "prehensile" and figures (pl. 31, fig. **168**) what is obviously the other appendage under the title "second antenna".

The mouth is placed more anteriorly than is usual in copepods (under the median part of the labrum in Fig. 11). In ventral view the labrum forms a shallow, wide area with thickened exoskeleton; its posterior edge is broken by three projections. The middle one of these has heavily sclerotized sides but a thin ventral surface, so that it generally appears medially incised rather than, as is in fact the case, posteriorly acuminate especially if viewed from behind instead of ventrally. In the latter aspect it may be seen to be divided for most of its length by a line parallel to the sides. This medial structure appears to correspond to the labral area of more typical poecilostomes, while the two lateral "hooks," which extend backward nearly

to the maxillae, are a distinctive feature of *curticaudatum*. Under each is a local modification of the ventral exoskeleton (its outline shown dashed on the right side of Fig. 11) which fits into the depression in the anterior surface of the mandible. A fleshy, protuberant structure arising just posterior to the angle of the maxillae (and presumably preventing excess posterior movement in these) is believed to represent the labium. The heavily sclerotized borders around the bases of the maxillae fuse medially to delimit the labium from an anterior, triangular area which continues uninterrupted to the mouth. In it, at the level of the median maxillar insertions, is a longitudinal ridge bearing, like the surrounding parts of this surface, a few tiny spinules. Nothing resembling characteristic paragnaths could be found but two small flaps clothed in short cilia occur—one on either side of the midline—under the inner lobes of the maxillules (and thus hidden by them in Fig. 11).

The mandible is oriented with its flat surface almost parallel to the plane of the mouth region, the main shaft extending forward at an angle of about 45° to the transverse and the tip curving backward (Fig. 11). Terminally (Figs. 3, 4) it bears a stout curved spine, with a toothed flange on its ventral side, and a short fimbriate lamella, both articulating with the body of the appendage and pointing somewhat posteriorly. As is typical in clausiids, the appendage is small and, in an undissected specimen, very difficult to distinguish under the labrum and inner lobe of the maxillules.

The maxillules (Fig. 5) are unimerous but bilobed: the outer, bearing three setae, is thinly sclerotized and protrudes in a transversely posteroventral direction from the lower side of the appendage, while the two setae of the inner lobe —one covering the other in a ventral view— extend toward the mouth. One or more of these setae on both lobes sometimes could not be discerned: this is presumed due to variation, to the hazards of dissection, or to difficulties in observation.

The maxillae (Fig. 6) insert over a considerable area of the cephalosome. They appear to resemble closely the same pair of appendages in *Clausia* (Giesbrecht, 1893), and, like these, their segmentation is difficult to distinguish. It seems probable that each has two podomeres, the basal being greatly expanded, the distal inserted eccentrically upon it and bearing a thickly spinulose dorsal lobe. Neither podomere is armed in the conventional fashion, but it is possible that the dorsal lobe, which appears to articulate at its base, represents a modification of the terminal armature. One may speculate that, by grinding the tips of this pair of appendages together, the animal could triturate the relatively soft skin of its host and feed on the resultant debris.

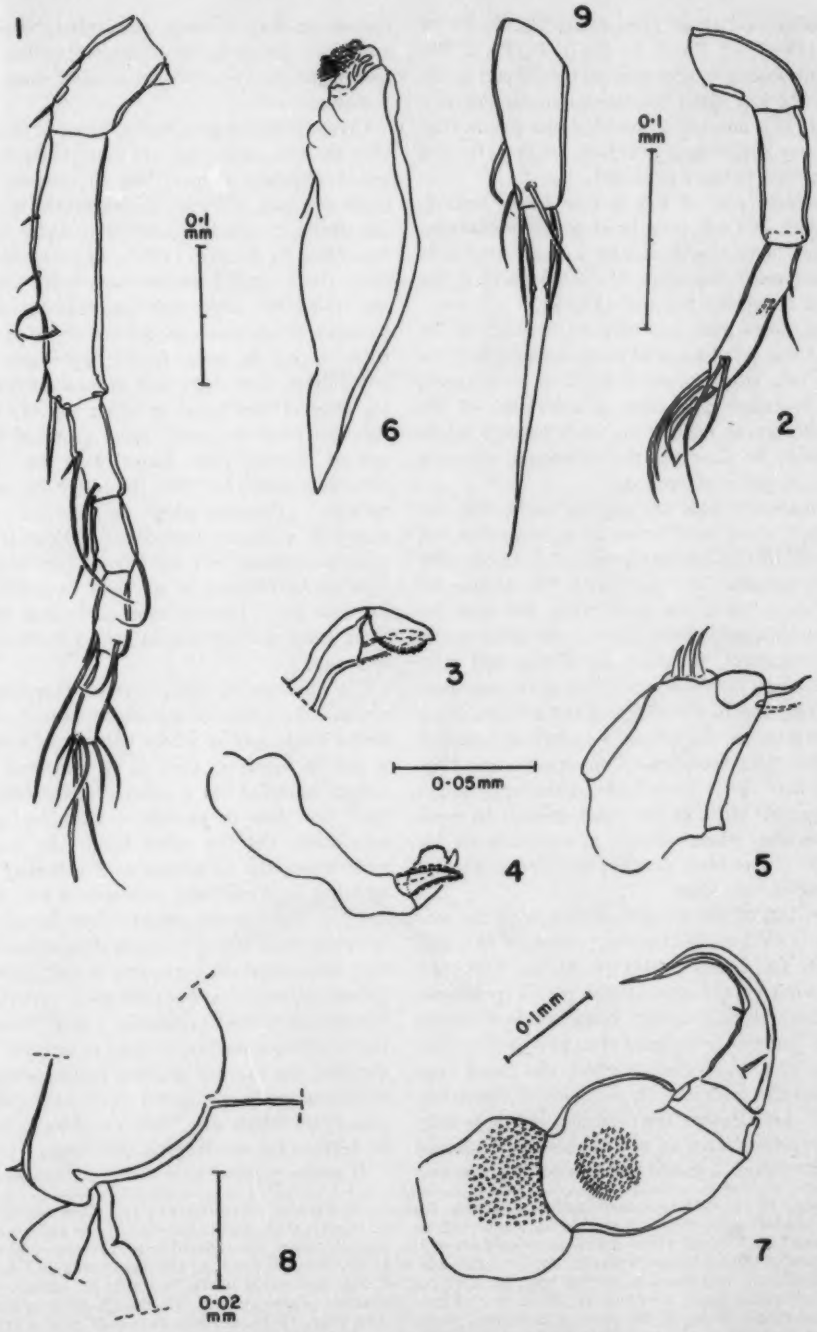
The maxillipeds form the main prehensile apparatus. Each is strongly developed and tetramerous (Fig. 7). The first two podomeres are somewhat inflated and the fourth produced into a strong, tapering, hooklike structure. Patches of fine spinules on the inner surface of the first and second podomeres represent the only ornamentation. The armature consists of a short seta on the inner surface of the fourth podomere and a small element near the inner curvature of the hook.

Despite Wilson's statement (1944: 546, 547), there are only three pairs of legs on the metasome, no trace being found of the fourth. Each is small and is borne just anterior to the middle of its respective somite. The legs are biramous, the two rami and the protopodite having two podomeres respectively, although the division between those of the endopodite is sometimes very difficult to distinguish. The armature is somewhat irregular: the basic pattern appears to be:

	prot.		*endp.*		*exop.*	
	1	2	1	2	1	2
Leg 1	– –.	I 1.	1 –.	6 –.	– I.	4 IV.
Leg 2	– –.	– 1.	1 –	5 –.	– I.	4 III.
Leg 3	– –.	– 1.	1 –.	4 –.	– I.	3 III.

but, as shown in Figs. 12–14, this may vary even between the legs of a single pair. The main differences occur in the second legs, where the last podomere of the endopod may have four (Ph. 2, left leg), five (Ph. 1, L; Ph. 3, both legs; Pe. 1, B; Pe. 2, L), or six setae (Ph. 1, R) and that

FIGS. 1–9.—*Pherma curticaudatum* Wilson, female: 1, Left antennule, ventral; 2, left antenna, dorsal; 3, tip of left mandible, ventral; 4, left mandible, dorsal; 5, left maxillule, dorsal; 6, left maxilla, dorsal; 7, right maxilliped, medial aspect; 8, right sixth leg and adjacent structures, ventral; 9, left caudal ramus, dorsal. (Figs. 1, 2, 4-6, 9, and 12–16 are of Ph.1; 10 of Ph.3; 11 of Pe.1; and 3, 7, and 8 of Pe. 2. A camera lucida was used for Figs. 10 and 11; the others were drawn with a carbon-arc type of projection apparatus. Scales refer to the figure (or figures) nearest them, and were made from a stage micrometer.)



FIGS. 1–9.—(See opposite page for legend).

of the exopod three (Ph. 1, R; Ph. 2, L) or four (Ph. 1, L; Ph. 3, B; Pe. 1, L; Pe. 2, R). It is interesting to note that on the exopod spines of all the legs distal setules—so characteristic a feature in a number of poecilostome genera that they may represent a primitive tendency in this group—are present (Fig. 15).

The fifth pair of legs is completely lacking, but sixth legs are considered to be represented in some of the specimens by a small stout seta just anterior to the origin of the ovisacs on either side of the genital segment (Fig. 8).

Male.—No male was present in either of the collections, and I know of no account in the literature of a copepod which could be so assigned. This is unfortunate since a knowledge of the morphology of males from each locality might do much to clear up the taxonomic dilemma posed in the next section.

Remarks.—There seems little doubt that the genera *Pestifer* and *Pherma* are synonymous, but I should like to consider briefly the reasons why I have identified *Pe. agilis* with *Ph. curticaudatum*. As noted in the description, the available specimens exhibit differences in the armature of the antennules, antennae, maxillules, and metasomal legs, in the segmentation of the antennae and legs, and in the shape of the ovisacs. None of the first six characters is apparently consistent within the specimens of one species, and variation may occur even between the appendages on opposite sides of the same animal. In some of the cases where changes in armature are involved it has been possible to infer accidental loss of certain setae.

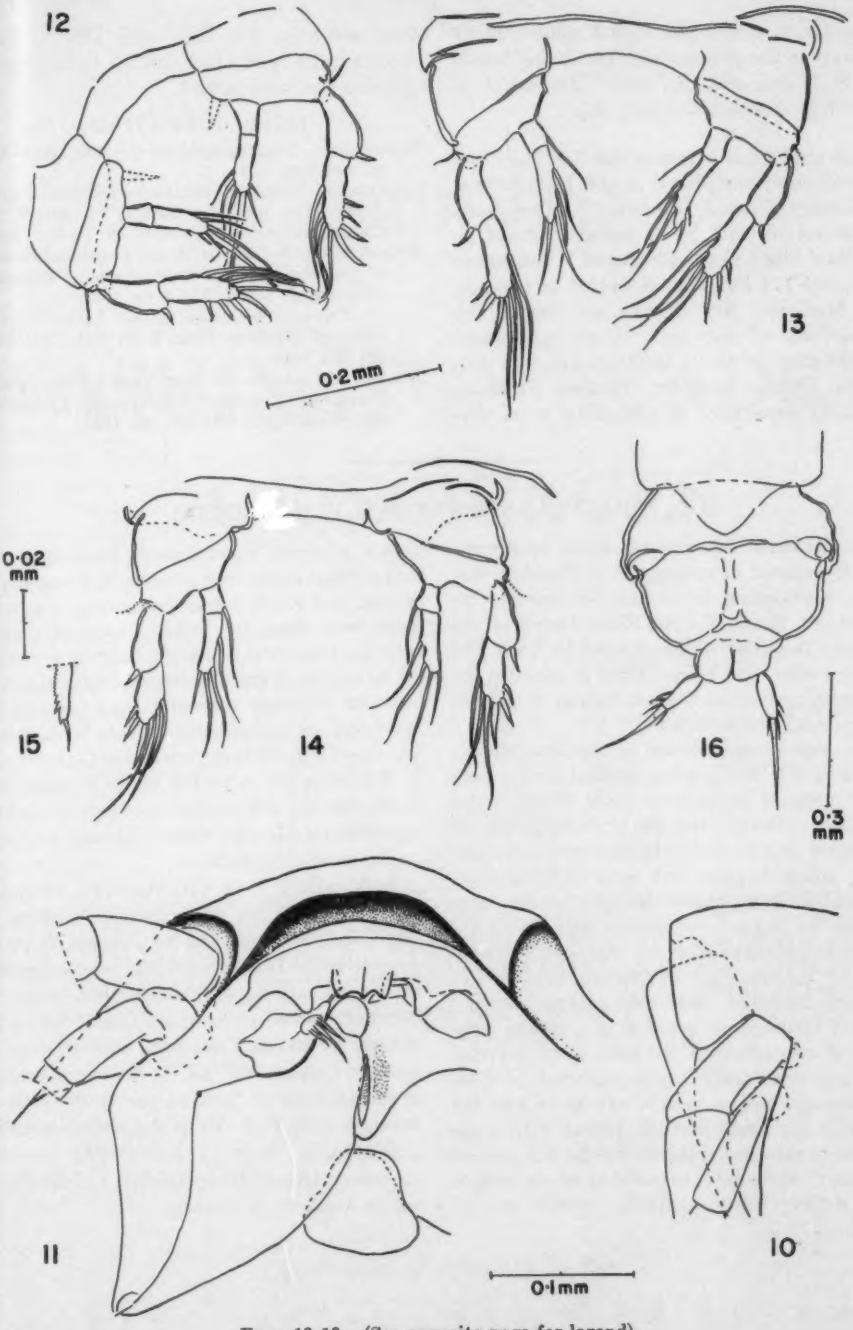
The last of the differences (shape of the ovisacs) is evident from a comparison of Wilson's (1923) fig. **1** and (1944) pl. 31, fig. **165,** and borne out by my examination of his specimens. Although there is usually considerable variation in the form of the ovisacs even in a single population of a particular copepod, the mean may prove valid as one of the complex of characters which distinguishes that species. But here only one representative in each collection possessed intact ovisacs. I should not like to diagnose two species on such a basis, particularly since the specimens are from different geographical areas and might thus be expected to show some slight variation.

Comparative zoogeography, however, suggests that the two collections are more likely to represent different species than to compromise a single one and, although this argument is purely inferential, it seems suitable to consider it here. According to Ekman (1953), archibenthal animals (both *curticaudatum* and *agilis* fall into this category) show only limited distribution patterns. If one assumes that the depth at which these copepods were found represents their lower limit, then there still exists evidence that the tropical shelf fauna on either side of Central America possesses many more pairs of closely related species than forms with an amphi-American range, and that the latter are all very ancient. (Paleontological information about copepods is almost nonexistent.) Even if these copepods extend into the abyssal region, a continuous distribution is not very probable. Nor do their hosts provide more assistance on this topic since neither was identified further than to group.

The position is, then, that on morphological grounds the available material appears to represent a single species whose phenotypic variation is not in excess of that to be expected in an animal modified for a strictly associated existence, nor does it provide a basis for specific separation. On the other hand, the locations from which the specimens were collected might lead one to expect the presence of two similar species. The number of specimens is too small to decide conclusively between these alternatives. But, since copepod taxonomy is still dependent almost entirely on morphological criteria, the former has seemed preferable. I shall thus leave to some future worker, with more material at his disposal, the onus of proving that speciation or subspeciation has occurred in *Pherma curticaudatum* (in which case Wilson's name *agilis* may be revived for the West Indian form).

It seems possible now to refer *Pherma* to the

FIGS. 10–16.—*Pherma curticaudatum* Wilson, female: 10, View of the anterior part of the cephalosome from the left side, showing groove between rostrum and mouth area, and insertions of the antennule and antenna; 11, ventral view of the anterior part of the cephalosome: the mandible and framework around the base of the maxilla are shown on the right side of the figure; the bases of the antennule and antenna, the maxillule and maxilla on the left; 12, first pair of legs and coxal plate, ventral; 13, second pair of legs and coxal plate, ventral; 14, third pair of legs and coxal plate, ventral; 15, detail of tip of terminal exopod spine on leg 3; 16, part of urosome, ventral. (In Figs. 12–14, certain elements of the armature whose absence in the specimen from which they were drawn is presumed to be accidental have been indicated by dashed outlines. Figs. 10 and 11 are somewhat diagrammatic.)



FIGS. 10–16.—(See opposite page for legend).

Clausiidae, a step which makes some changes necessary in the present definition of that family (M. S. Wilson and Illg, 1955). Treatment of this will be deferred to a later paper.

This study was begun at the U.S. National Museum and completed in the Department of Biology, Boston University; it has been supported in part by a research grant to Dr. Paul Illg from the State of Washington Initiative 171 Fund for Research in Biology and Medicine. My thanks are due to the authorities of the institutions mentioned for the use of their facilities, and to Dr. Libbie Hyman and Dr. Thomas Bowman for their assistance in obtaining type material on loan. Dr. Illg and Dr. Arthur Humes have been kind enough to read and criticize the manuscript.

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DR. FRIEDMANN AWARDED ELIOT MEDAL

DR. HERBERT FRIEDMANN, acting head curator, department of zoology, U. S. National Museum, Smithsonian Institution, has recently received the Daniel Giraud Elliot Medal of the National Academy of Sciences for his book *The honey-guides.* The Elliot Medal is awarded for the most meritorious work in zoology or paleontology published each year.

Dr. Friedmann's studies of the honey-guide, issued by the Smithsonian, clarified several puzzling problems about these birds. Prior to this work it was thought that the birds fed chiefly on the honey and bee larvae in wild bees' nests, but being unable to open such nests by themselves, the birds led or guided humans to the hives (hence the name honey-guide), and then after the men had taken their fill, they came back to feed on the remnants left strewn about. Since the only use to the bird of the guiding habit depended upon the cooperation of a totally independent creature, man, the habit could not have had any value until it was perfected by both participants, and in such a way as to help the birds. It was found that the African natives deliberately substituted themselves for the original "partner" of the bird, the ratel or honey-badger. The guiding behavior, which appears so purposive, is merely an excitement reaction of the bird when it meets with a potential foraging associate, and which calms down when it sees or hears bees. Since this usually happens near a bees' nest, the effect is that the follower is usually led to a hive. Many observations show that the behavior is purely instinctive and involves no "planning" or preknowledge by the bird. It was also found that the birds' interest in the hives was in the wax of the comb not in the honey or bee larvae. Studies showed that the birds depend on wax-splitting microbes in their digestive tracts to make the wax digestible.

Born in New York City April 22, 1900, Dr. Friedmann took his undergraduate training at City College in New York and received his Ph.D. in ornithology from Cornell University in 1923. He was a National Research Council fellow at Harvard University from 1923 to 1926, an instructor at Brown University, 1926–27, and at Amherst College, 1927–29. He has been a curator in the Division of Birds at the U. S. National Museum since 1929. He is the author of many ornithological works, including *The parasitic cuckoos of Africa,* Monograph No. 1 of the Washington Academy of Sciences.